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American Anthropologist

NEW SERIES

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# OPPORTUNITIES FOR COÖRDINATION IN ANTHROPOLOGICAL AND PSYCHOLOGICAL RESEARCH[1]

BY CLARK WISSLER

THE creation of a joint Division of the National Research Council to represent anthropology and psychology brings us together here in order that we may consider how and to what degree the aims of both may be coördinated. I have been asked to outline some of the problems that invite such coördination, but before entering upon the specific discussion of opportunities for coördination and coöperation, some general orientation seems necessary. In the first place, we are dealing with two distinct sciences, each of which grew up in its own way and each of which shows every indication of being able to stand upon its own feet. These two sciences differ not only in their points of view, but also in their major contents. About the only ground they seem to have in common is that they both study men. It is true that psychology gives a large place to the study of animal intelligence, but on the other hand, anthropology also concerns itself with morphological problems among the higher mammals and even dips into the pairing and other social habits of the anthropoids. There is, however, a difference in that anthropology is at present largely concerned with the morphology of the case, while psychology puts its emphasis upon what it chooses to recognize as intelligence. Yet this distinction should not obscure the other relations and attitudes of the two sciences. Thus, psychology gives a great deal of attention to the ways of doing things, learning, habit, etc.; and again, anthro-

[1] Address before the joint session of the American Psychological Association and the American Anthropological Association, at Harvard University, December 30, 1919.



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pology is interested in similar problems, for it is taking infinite pains to trace out human tool concepts and other fundamental processes from the beginning of palaeolithic time. It has gathered from the remote corners of the earth data on the concepts and habits that underlie these processes. In fact, the anthropologists are gradually putting together the facts that are to constitute the history of human psychical functions from the beginning to the present. I predict that when that story begins to be rounded out psychologists will find it one of the most fascinating chapters in science. Anyway they, and they alone, will be able to interpret it in terms of functioning individual intelligence. Thus it is apparent, that the common tendency of the two sciences to study men and their performances, does bring them into direct contact at many points where a full interpretation of the results obtained in the pursuit of one science depends upon the insight obtained in the other.

But notwithstanding these obvious overlappings there is one difference in which lies the clearest and most tangible distinction between the two sciences. Psychology takes as its unit phenomenon the mind of man, whatever that may mean. If I were speaking anywhere except among the greatest psychologists of the world, I should know exactly what was meant by the mind of man and should expect no challenge, but here it is well to be cautious. Yet, one thing we can be sure of and that is, that psychology is concerned with a group of functions that center in a man. His individual performances are always the point of departure.

Anthropology, on the other hand, takes the group as its unit and point of departure. It is not greatly concerned with the function of the individual in the group. In fact, if the anthropologist did center his interest on the individual in the group he would soon be indistinguishable from a psychologist. The anthropologist is not interested in the problem as to how the individual fits himself into the group, how he learns the tasks required of him by his group, or even with his inherent specific reactions to the life of the group. He is, however, vitally interested in what the group requires of the individual and by what steps the group came to exact these requirements. Thus the psychologist may be interested in the successive

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functional processes by which a child eventually acquires the art of writing a letter; the anthropologist, on the other hand, cares nothing about that, but seeks to know what brought the group to the formalization and exaction of such a requirement, to which end he compares groups both as to their performances and as to their organic constituents. In more general terms the psychologist deals with what goes on within the individual when confronted by the group and the environment, while the anthropologist gives his attention to what goes on in the group when confronted by other groups and environments. These distinctions would be absolutely clear cut, if individuals did not constitute the group and so prevent one fixing his attention upon the group exclusively. Without the anthropological eye it is often difficult to see the group instead of the individuals composing it.

The anthropologist is put to great trouble to classify his groups, or units. So far, this has occupied a large part of his time; but in the study of the group he is for the most part seeking its origin and so puts more emphasis upon the genetic history of the group than does the psychologist upon the genesis of the individual. Again the anthropologist has a two-sided problem in the group, he seeks to comprehend it zoölogically on the one hand, while on the other, he deals with the psychic functional history of the group. He recognizes in the latter what he calls the *culture* of the group. By that term he means all social activities in the broadest sense, such as language, marriage, property system, etiquette, industries, art, etc.

It is a curious fact that the men who are most deeply engaged in a science are the very ones who are least able to give a clear-cut statement of its limits. This lack of precision may be the result of contentions among ourselves as to the precise limits of our definitions. But we should not permit our internal clashes of opinion to obscure and distort the fundamental pervading distinction between the objectives of the two sciences. The anthropologists are not unaware of the conflicts in the psychological camp, the noise of the battle has at times been audible from afar. Some of us have seen the old and time honored human soul forcibly

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ejected from the psychological domain; later we heard that psychology had lost consciousness; and now the report is that it is losing its mind. Nor have we escaped hearing about the wavering fortunes of the behaviorists, etc.

On the other hand, the psychologists cannot be entirely ignorant of the lack of harmony in the anthropological camp. They have doubtless heard the battle cry of those who believe the cephalic index to be the type of a universal index by which even differences in human performances may be explained. Again, they may have heard of the unrelenting fight between the evolutionists and the supporters of the historical method. Also the somewhat acrimonious contention over the functional potentional equality of racial groups, there being those who believe that the observed differences are due to convention and fortuitous events. Finally, the psychologists may have taken some casual interest in the heralding of a new onslaught against scientific tradition under a banner labeled the superorganic. Perhaps when we get a clear view of this new anthropological doctrine and tear away the camouflage, we shall find under it the poor old discarded soul of the psychologists.

It is to be hoped that the members of our division will have just enough of the sense of humor to get the true perspective of these necessary and healthy struggles, so that they may keep their eyes fixed upon the fundamental objectives of the two sciences, which in the terms of everyday speech are *mind* and *race*, respectively. At any rate, if we now keep this main distinction in mind, we can see the full justification for a joint division comprising psychology and anthropology. We have at least one common objective, *viz.*, racial characters. These characters may be anatomical, mental, and cultural. Thus it is that the results of psychological tests upon the racial elements in the army are of the utmost importance to anthropology. On the other hand, the parts these new-found degrees of capacity may play as factors in the culture complex of the group is the problem of anthropology and the final results of this investigation should also be of great import to psychology. The situation can, then, be stated as follows: it must be the policy of the Division to support jointly the development of each science

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independently, but in addition, the Division as a whole must make it its special business to throw its united strength upon the study of racial and social groups.

Since I have been asked to speak of possible ways of coöperation and coördination in the work of the Division, I need not discuss the special problems of psychology on the one hand, nor those of anthropology on the other. It seems unnecessary to say that when we speak of coöperating, we do not mean that anthropologists shall do the work of psychologists, and *vice versa*—nothing of the kind. It should be obvious that there are many important problems in anthropology that make no appeal to psychologists. For example, it would be too much to expect psychologists to care about the excavation of a shell-heap in Patagonia, or to show enthusiasm for a study of the relationship systems for Melanesia. Yet the pursuit of these problems may mean a great deal for the future of anthropology. Likewise there are numerous psychological problems of great import to psychology, for which no anthropologist will show much in the way of appreciation—for example the behavior of a white rat in a maze. There is no reason, however, why each half of our division cannot give sympathetic support to what the other considers of vital import to the development of its science. Hence, we have now to consider only such problems as fall within the domain of the common objective. As we have just seen, this common objective is the study of human groups—racial, cultural, and mental.

However, at this point it may be profitable to turn back and again contrast anthropology and psychology from quite a different angle. Instead of considering the main objectives of the two sciences, let us look into their histories and accomplishments. The "new psychology" of twenty-five years ago, now the psychology you profess, has been from the start a practical science, potentially so, if not actually. No doubt many of you will resent this statement, but the facts in your history cannot be denied. Those of you who were in educational work thirty or more years ago know how the whole teaching profession of the country took to psychology as an applied science. It is not strange then to find it one of the

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leading subjects in normal schools, training colleges, and schools of education. The old and long established psychology of the university could do no other than yield to this great external pressure for applied psychology. The history of the case need not be recited to you, it is obvious that the prestige now held by psychology is due to its achievements as an applied science. "Mental engineering" is now a favorite slogan, and everyone knows that the term engineering implies applied science. I do not wish to be understood as denying that psychology has not maintained its position as a pure science, I desire only that all of us take account of the strong development of applied psychology and the consequent richness in the psychological personnel and resources.

Anthropology, on the other hand, has so far stood as a pure science. It has not been the source to which the teaching profession or any other profession looked for guidance. Even today the number of our universities and colleges maintaining strong departments can be counted on the fingers of one hand. Research in anthropology has been supported almost exclusively through museums. That anthropology has been essentially a museum growth is clear when we note that even in the few large universities with departments, these departments were the outgrowths of university museums. It is not far wrong to say, then, that most anthropologists of the immediate past made their living as museum housekeepers and gave what spare time they could to the development of anthropology as a pure science. The result of this is a limited personnel and material resources. Had there been a vast professional host knocking at the doors of our museums and demanding practical guidance in their everyday work, there would have been a different story to recite here. Further, if I mistake not the signs of the hour, those who stand here ten years hence will have a far different story to tell.

But, it may be asked, what have these museum men been doing behind their closed doors? This is easily answered; they have been face to face with problems of race. They have developed techniques for dealing with the zoölogy of man and also with his culture. Yet the subjects of their investigations have been the

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lowly and backward peoples of the earth. On this account, anthropologists have been frequently denounced. Not so very long ago I heard a distinguished scientist say something like this, "The trouble with these anthropologists is that they while away their time studying Indians, Negroes, Bushmen, and other savages, when they should study Europeans: Europeans are the only people that count."

Now there are several reasons why the anthropologists have studied these "lowly peoples." For one thing, nobody objected. There were beneficent gentlemen willing to pay for this sort of thing, but who would not stand for having their families or friends investigated. Cities would support museums of anthropology so long as that subject was not in any way connected with the lives of their citizens. So the anthropologist bided his time. If any of you doubt his industry begin to round up the literature of the subject and to study a large museum. You will find that practically no savage group has escaped him.

Also, the anthropologist wished to be humane; his position was not unlike that of the much abused "animal experimenter" who first "tries it out on the dog." Well, he now has a profound knowledge of "the under dog"; he has confidence in the technique he has developed and his hands have long been itching for a chance to lay hold of Europeans and their cultures; in short, the anthropologist has arrived at the table of the National Research Council, dropped thereon his instrument case and announced that he is ready for the patient. Just how the psychologists will receive the newcomer remains to be seen; they have been at the bedside already, taken the first steps in diagnosis, and show some tendency to regard the case as their own. In proof of this, I may be permitted to quote a few words from the Proceedings of the Psychological Association at Baltimore, just one year ago:

> The course of events has put America under bonds to find and develop the social and mental factors that make for a stable social equilibrium. This is peculiarly the job of American psychology.

Another speaker forcibly defended the thesis that, "The future of the world depends upon the American psychologists."

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My citation of these words from these very eminent psychologists is not to scoff. On behalf of the anthropologists, I take this opportunity to congratulate the psychologists upon their glorious achievements in the war and to express our joy in the confidence with which they now turn to the gigantic problems of peace.

But what are these great problems the psychologists are to attack? They seem to be the problems that arise in such phenomena as "social unrest," "profit-sharing," "shop-management," immigration, vocational training, Americanization, international relations, etc. Certain it is that if psychology is to save the world, here is where she must work.

The psychologists seem to assume that the road to the solution of these world problems leads out directly from their own beaten track. Perhaps they feel confident that they can meet the situation by controlling the reactions of the individual. They may be right; but as an anthropologist, I sound a warning. The phenomena we have cited are to a large degree racial, they are moreover phenomena of culture. In short, the psychologist is about to attack a problem in culture and there are but two roads to success in this undertaking: one is for him to call upon the anthropologist for help, the other is for the psychologist to turn anthropologist. To take up one of these intricate problems in our culture without resort to the technique developed by the anthropologist, would be just as absurd as to proceed with a psychological problem, with only the technique of anthropology. The social body to which we belong may be sick, in fact I think it is, but it needs something more than psychological treatment.

We seem to be entering upon a new phase of the world's history. Some centuries ago the English people became conscious of the fact that they had a society, that society could be improved or rationally manipulated by the use of scientific knowledge. This was a great achievement and is regarded as the foundation of modern life. Now we are about to make another advance; the English-speaking people are about to become conscious of having a culture, conscious of the fact that each racial, or biological group has its culture. Then will come the realization that problems in culture

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can be met by the application of the appropriate scientific technique.

I repeat then, that the anthropologists are ready. Unlike the psychologists they have not had their chance; the hope is that it will come through this new organization. We look to the psychologists with their long experience in human engineering and their large and varied personnel to lead out. Of specific problems there is no end.

For example, many years ago an anthropologist discovered some curious retarding influences when he compared the statures of children in a charitable institution with those living at home.[1] His suggestion was that some forces might operate in institutional life to retard development. The problem, then, is do children in even the best of charitable institutions grow as we think they should? Let us as psychologists and anthropologists attack this question to find out what zoölogical and psychological factors are involved here. Suppose, in short, that we investigate the whole case of institutional life to see what is happening to these children. This would present a fine opportunity for the psychologists and anthropologists to work side by side, but above all, to render a great public service.

Again, we hear a great deal about Americanization and Americanization programs. The idea in this is to make one people of all who reside among us, but few will go so far as to say that this implies amalgamation. What is really meant is complete culture uniformity. Hence the problem in Americanization is to bring about the complete adoption of our culture on the part of immigrants of different cultures.

It is common knowledge that immigrants from cultures obviously different from our own settle in colonies where they maintain their native languages and customs. Some necessary adjustments seem to be made to the political and economic complexes of our culture, but otherwise the group exists as an area of foreign culture. This is recognized in popular speech when we speak of "Japanese colonies," "Italian colonies," "Armenian colonies," "Finnish colonies," etc. In such colonies we recognize a menace to our own

[1] A. Hrdlička, *Investigations on One Thousand White and Colored Children*. New York, 1904.

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culture and national existence and Americanization is the general term for all efforts looking toward the substitution of our own culture for that of the "foreign colony."

The initial difficulty in all programs of Americanization comes from our lack of specific knowledge as to what goes on under the surface in these groups. No anthropologist has investigated these "colony cultures," yet there are fifty or more Indian colonies in the United States and Canada for anyone of which you can obtain a publication in which there will be found an exposition of its culture based upon investigations by anthropologists. Thousands upon thousands of dollars have been contributed by wealthy men to this end, not to mention large sums spent by the U. S. Government through its scientific bureaus. I need not mention that for the native tribes of South America, Africa, Australia, etc., we have similar studies. It seems high time, therefore, that we formulate some knowledge of the "foreign culture" colonies in our midst, before we set out gaily upon a great program of Americanization. Anthropology is ready to make the necessary investigations, if the opportunity is created.

However, the problem is not merely one of culture, for the zoölogical factor is evident in the "race question." The leveling down of differences in culture means increased contact and eventually amalgamation. It is highly important, therefore, that we study the inherent factors in the population of these colonies. We must also take up the problem of race-mixing; someone must be able to tell us what kind of men are being produced by this inevitable crossing of racial elements. Here are great groups of problems:

1. The identification of racial characters.
2. The inheritance of morphological and mental characters.
3. The effects of external conditions upon individual development.
4. The psychological factors involved in culture change.

These problems appear in another question of large proportions, *viz.*, the Negro question, or rather the mulatto problem. The psychologists have made a good beginning with their tests upon

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soldiers and great things are to be expected from them in the future. What we need now is a start on the anthropological phase of the problem.

Again, applied psychology has come to hold a definite place in industrial management. Large technical schools now feel called upon to apologize if that subject does not appear in their catalogues. But when these schools send their students out to fill factory positions, they will find the racial and cultural factors ever to the fore. Of these psychological engineers we have often heard, but the anthropological engineer is also a reality; a cotton-producing corporation using native labor in several countries employs an anthropologist to show how to handle these different culture and racial groups; another anthropologist is employed by an immigration commission; etc. The textile and clothing industries are beginning to employ anthropologists to assist in training their workers and also to develop certain aspects of the industry. Since the psychologists are well advanced in the development of industrial service, it would seem advisable that our division support a committee to develop anthropological service to industry.

I think enough has been said to show in what direction our joint opportunities lie. If one of the aims of the National Research Council is to make science an effective leader in the problems of peace, then someone must take up the problems we have outlined. The opportunity seems at hand for investigations upon a much higher plane than in the past and if we accomplish nothing, we have only ourselves to blame. I know there are anthropologists who look with disfavor upon the present form of our division. They are opposed to any and all efforts looking toward coöperation and coördination. Their idea is that we should have two separate sections; in other words that we should turn our backs upon each other and set out alone. I infer that there are psychologists taking similar views. These opponents of the present policy of the division believe that the objectives and points of view of anthropology and psychology are incompatible; that there may be common ground between them, but that coöperation even here is impossible. The course of events may prove this to be correct; but

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if so, the important problems we have cited will fall to no one; anthropology will become even more than before merely the detached study of the lowly and obscure peoples of the earth, her energy dissipated by internal quarrels as to the relative values of the zoölogical and cultural methods. She will lose the stimulating contacts with the large and diversified personnel of psychology and so suffer more and more the evils of isolation. On the other hand, I believe that psychology will also be the loser. She will for one thing ignore the experience and technique pertaining to a phenomenon, some aspects of which can with difficulty be distinguished from what she regards as her own. Further, she will ultimately find herself forced by public demand to take up problems of race and culture and thus to duplication of effort. As I see it the two sciences have nothing to lose and much to gain in a united effort. But far above our narrow personal interests are the needs of the nation. The hope of mankind is that science will point the way to correct procedure even in matters of education and social adjustment. The power of science, when its efforts are coördinated, was clearly demonstrated during the war. It needs no defense now. It is for psychology and anthropology to live up to the reputation of science as a whole.

AMERICAN MUSEUM OF NATURAL HISTORY,
NEW YORK CITY.

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# THE PAPAGO HARVEST FESTIVAL

By J. ALDEN MASON

THE religious ceremonies of the sedentary pueblo peoples of the American Southwest are comparatively well known,[1] and considerable has been written on the ceremonies of the nomadic Athapascan tribes.[2] To the south the religions of the Cora[3] and Huichol[4] have been carefully studied and, to a less extent, those of the Tepehuane and Tarahumare.[5] But of the religious practices of the Piman groups in the region between, practically nothing is known, and it has been generally supposed that religious symbolism and ceremony were almost nonexistent among these groups. It is probably true that they are far less highly developed than among the before-named groups to north and south, but there is, nevertheless, a considerable body of religious practice as yet unknown to the ethnologist. In his voluminous work on the Pima Indians[6] Russell hardly touches upon the religious ideas and gives only one page to "festivals," mentioning the spring sahuaro festivals to celebrate the making of sahuaro wine, and the "Name Song" festival. Nevertheless it seems almost impossible that the Pima should not have some harvest ceremony analogous to the Papago Vigita.

[1] The list of authorities on the religious ceremonialism of the Pueblo peoples contains the names of the majority of American anthropologists. Among the most important may be named A. F. Bandelier, J. G. Bourke, F. H. Cushing, G. A. Dorsey, J. W. Fewkes, P. E. Goddard, F. W. Hodge, Walter Hough, G. W. James, Washington Matthews, E. C. Parsons, J. W. Powell, James Stevenson, M. C. Stevenson, and H. R. Voth. A compact bibliography of their works may be found in *A Bibliography of Arizona*, compiled by Hector Alliot, Los Angeles, 1914.

[2] Cf. supra, particularly J. G. Bourke and Washington Matthews.

[3] K. T. Preuss, *Die Religion der Cora Indianer*, Leipzic, 1912.

[4] Carl Lumholtz, "Symbolism of the Huichol Indians," *Memoir of the American Museum of Natural History*, vol. III, New York, 1900. "Conventionalism in Design of the Huichol Indians," *ibid.*

[5] Carl Lumholtz, *Unknown Mexico*, New York, 1902. Also minor articles mentioned in bibliography there.

[6] Frank Russell, "The Pima Indians," *Twenty-sixth Annual Report, Bureau of American Ethnology*. Washington, 1908.



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Carl Lumholtz in his recent book was, so far as I am aware, the first to call attention to the Vigita.[1] That the ceremony has remained little known is probably due to a combination of circumstances. The naturally more or less bare culture of the Papago, both from a material and a spiritual point of view, has presented an unattractive field to the ethnologist, as has his desert habitat to the white homesteader. And this is particularly true of Santa Rosa, the little village where the festival is always held, being, until very recently, the most conservative Papago village.

The following account of the Vigita[2] or Harvest Festival was secured in January, 1919, at Santa Rosa where the writer was engaged in pursuing linguistic studies on Papago for the Southwest Society of New York and the University of California. It is with their permission that the data are here presented. Practically the entire series of notes and songs was secured from José Juan, one of the singers at the festival.

The festival is held the last of November, supposedly every four years. But it depends largely upon the success of the harvest of the crops and it has now (1919) been six years since it was held. It may be held this year or may never be celebrated again, as the establishment of a government school, public power pump, and trader's store at Santa Rosa will naturally result in a speedy loss of conservatism.

The celebration of the Vigita is vested in the five principal villages of the Santa Rosa valley, though celebrants attend from all the villages north of Indian Oasis and ally themselves with one or other of the five. These five are koa'tci (Achi), ko'kema't ke''k, kai'tcima'k (Santa Rosa), aktci'n (Akchin) and a'nekam (Anekam).[3] The festival is always held at Achi which is considered the foremost Papago village; it is now considered as a part of Santa Rosa, being only about a mile to the northeast. Anekam is several miles to the north and Akchin several to the south. Kokematkek is in

[1] Carl Lumholtz, *New Trails in Mexico*, New York, 1912, p. 92–98.

[2] "The name is derived from the word *viiki*, by which is designated the finest and smallest plumes of the vulture." Carl Lumholtz, p. 92.

[3] Lumholtz states that four groups take part in the festival: Santa Rosa, together with Sikulhímat, Alóitak and Kvítatk; Kvívo and San Xavier; Ánekam; Aktjin. Cf. p. 96.

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the near neighborhood but the exact location was not learned. The date of the celebration, however, is set by the principal men of vai'num ke''k, Pump House or Iron Pump village. This is said to be due to the fact that a few years ago the most important men left Santa Rosa and founded this new village near Quijotoa where a mining company had erected a pump.

The elders at Iron Pump village, having decided that the time is ripe for a celebration of the Vigita, journey to Achi and a council is held at which time the exact date is set. This journey is made and the council held at night and in secret session, and all details are settled before the public announcement is made. The date set is generally twelve days after the meeting of the council, about the end of November.

The day after the council, the entire population of the twin villages busy themselves cutting bundles of sticks which are left to dry for a day. The following day they are shaved and cut to a uniform size, about a foot in length, adorned with fine turkey (sic) feathers on one end and tied up in bundles of about 8 inches in diameter. These sticks are painted blue or green and represent the growing corn, the turkey feathers being the leaves.[1]

Early in the evening of the tenth day before that set for the celebration, all the bundles of sticks are gathered into one great bundle and set in the center of the feast ground. A fire is built near it and around this all the men gather. Then the chief from Iron Pump village makes a speech. This is a set traditional speech, telling how the Vigita originated and how it has been celebrated from remote times. After a short pause a second speech is recited, setting the date for the festival. After these two speeches the people return to their houses, those from distant villages going to camps in the neighborhood. Two old men, the chiefs of Iron Pump village and of Achi, guard the fire and the sticks all the following day. In addition to the great bundle of prayer-sticks, ten tally-sticks which are preserved from year to year are stuck in the ground near the fire and every evening one of them is pulled out by the

[1] Evidently the typical "prayer-stick" of Northern Mexico and the American Southwest.

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chief of Achi on going home. This keeps the tally of the days and when only one stick remains, the celebration is held the following day. Every evening when the tally is pulled, the chief of Achi takes the great bundle of prayer-sticks and carries them slowly and solemnly to his own house outside of which a fire has been built.

The first night, after sundown, all the men gather near this fire and the speeches of the previous evening are repeated. Then all arise and go again to the dance-ground. At some distance a man has been hiding with a bull-roarer, *wewegita*,[1] who follows them swinging this, thus imitating the sound of the rain. On arriving at the dance-ground they sit around until after midnight when one man starts for Iron Pump village with a small bundle of sticks to announce to the people there the date set for the Vigita. Early in the morning another man goes to Akchin and Silnaki and when the former messenger returns from Iron Pump on the morning of the following day he goes to Anekam with another bundle of sticks.

The intervening days are spent in elaborate preparations for the feast. Only men take part in the actual ceremonies. Before the day of the festival only the principal men come from the more distant villages, but from the nearer ones all the men assist in the preparations. But as the time approaches, families begin to arrive, most of them coming two days before, and each one immediately comes to the dance-ground to introduce himself and to get his prayer-stick. Every evening when the chiefs come home they

[1] "An important part of the singer's outfit is the bull-roarer, consisting of two flat pieces made of sahuaro rib, the smaller one being held by the hand when in use. The connecting string should be twine of native cotton, which still may be found in use. They are decorated with symbolic designs, such as those standing for lightning, clouds, turtles, grains of corn, expressing their desire for rain. The buzzing sound produced should be deep, in imitation of the thunder, which brings rain; if the sound is shrill, lightning only will follow.

The bull-roarer is swung for the purpose of calling people together and as a sign that the meeting is over, both at the preliminary exercises in the practising enclosure as well as at the feast itself. These buzzing implements not only open and conclude the proceedings, but they are used on the way from the practising house to the feast early in the morning, also when the cloud symbols are carried about, and on similar occasions. When not in use, the implement is tucked under the belt at the back. After the festival it is put away not to be used until the next feast takes place." Lumholtz, p. 95, 96: plates opp. pp. 88 and 96, *i*.

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bring a small bundle of sticks with them and give one to each newcomer. Thus everyone receives a stick which keeps him from becoming ill at the celebration and which he takes home with him to serve as medicine. With it he brushes himself clean.

The greater part of the ten days is spent in learning and practising the songs to be sung in the ceremony. The men from each of the five villages build a small enclosure or corral which is the headquarters of this village and where the practising is done. As each group finishes its enclosure it sings a song to gain the blessing which is vouchsafed to the first village to complete its corral. Here the singers gather every evening and practise. They are known as *vi'pinyiṁ* (singular *vinyiṁ*) and form a numerous body, each village having as many singers as it can secure. There are eight principal *vi'pinyiṁ* from each village and an indefinite number of choristers. Each of the eight composes a song and teaches it to the chorus who do the actual singing at the festival, the composers not taking part. Each singer wears a mask made of a gourd painted in bright colors and carries a rattle.[1] There are thus eight songs sung by each village chorus at the Vigita and new songs are composed and sung every year. Every evening after the completion of the village enclosure the singers meet and practise their songs so that there will be no mistakes on the final day.

Another numerous body of men are the *nanawitcu* (singular

[1] ". . . a different kind of mask, neatly made from a gourd, and painted. It is worn by a singer at the same great feast. . . . There are three sections of colors on the singers' masks symbolizing clouds of similar hues. The upper part is painted with red ochre; then comes a black band which is produced by a mixture of sap from the mezquite and oxide of iron; the white band is made with chalk. The zigzags of the red section symbolize clouds, the dots are grains of corn. The designs on the white section denote clouds and lightning. The singers (vîinim) have the same kind of rattles as the clowns, consisting of a number of the small bags spun by an insect (*Attacus orizaba*), with a pebble inside of each, and attached to a band around the ankle. The band should be cut from the skin of a black dog, which is killed for the purpose in the practising enclosure.

" The singer has the upper body nude, his trousers being turned up as high as possible and his feet bare. He wears no head-dress, but attempts to appear neatly attired, tying around the loins a colored bandanna or perhaps a shawl borrowed from his wife. Around the waist, the neck, and the upper part of the arm bright-colored strips of cloth are tied. His body is smeared with red ochre on which are spots of white, symbolizing grains of corn." Lumholtz, p. 94, 95; plate opp. pp. 96, *a–d*.



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*nawitcu*) or clowns.[1] These represent sahuaros, the giant cactus, and wear turkey feathers on their heads to represent the fruit and carry rattles. They also carry long poles which represent the poles with which the sahuaros are gathered and hold representations of clouds made of cloth at the celebration. These *nanawitcu* go out among the houses and bring food and drink to the singers practising and to the head-men who are too busy to go home to meals. They also bring the wood for the fire and all other needed supplies. There are about ten of these to each village.

There are furthermore two men known as *tcu'i'wa'tam* (singular *tcu'i'wa't*) who probably represent shamans, one being stationed at Achi and the other at Santa Rosa. About three a.m. on the day following the erection of the village enclosures the two prepare to change places. While they are making their preparations the *vi'pinyim* stand near and sing. The *tcu'i'wa't* from Santa Rosa then goes to Achi where the people are waiting for him around a

[1] " . . . the clowns are the funny men of the occasion, and their apparel and weapons are in accordance with their functions. Their bows are crooked mezquite roots with strings attached. Their ridiculous-looking arrows, four for each, are made from sahuaro ribs, with turkey feathers as the plumed part; like the hunter, the clown has two kinds, but his are out of proportion, three being thin and one very stout. His bracelet may be a piece of unborn deerskin wrapped around the wrist. The most striking part of the costume is the mask, which is made of canvas, like a hood that is drawn over the head; formerly perhaps basket work took the place, at least in parts, of the canvas. Small holes for the eyes have been pierced in it and the top is adorned with a large bunch of plumes from the turkey, hawk, and a black sea-bird. "Horns" made of turkey plumes are attached to the sides, soft down from a hawk being tied to the top. The decorations on the facial part of the mask symbolize clouds. Under the clown's belt is tucked a wooden machete, and large strings of sea-shells run over the shoulders across the chest and back. He wears a huge tobacco-pouch and carries a sahuaro pole on which small greasewood sticks, tied at right angles, do duty as hooks.

"His bare arms, as well as his arrows, are decorated each with a spiral line made with chalk and encircling lengthwise, while his legs are daubed in spots, the color being afterward allowed to wear off. At the feast, the numerous clowns perform pranks everywhere; they visit the houses, offer food, and shoot at men disguised as deer. During the dancing they keep in the middle of the dancing place. They neither sing nor talk, though they may do so if requested. If a clown breathes on a sick man, the latter gets well. The mask when not used is kept in the house of the owner, usually in a covered earthenware jar. The dignity of the office, which does not imply the necessity of being a medicine-man, is confined to certain families, the father deciding which of his sons is to be the next clown." Lumholtz, pp. 93, 94; plate opp. p. 92.

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fire. Arriving there he sprinkles corn-meal on the breasts of all, thus preventing sickness and bringing them long life. Meanwhile the *tcu'i'wa't* from Achi does the same for the people of Santa Rosa. This is done for two mornings and the third morning the two perform the same rite in their own respective villages.

After nine days of preparation the solitary tally-stick in the

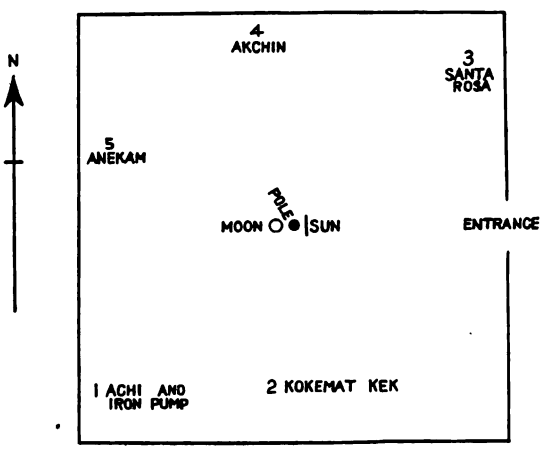



Fig. 1.—Diagram of the Vigita enclosure. The entrance and the pole with its ceremonial paraphernalia are shown with their proper orientation. The village groupings are shown in their proper relative positions, but the orientation is uncertain.

center of the dance-ground indicates that the following day is that of the celebration. On this day all the celebrants gather together and build the main enclosure (fig. 1). This is about ten paces square and built as high as the tallest corn-stalks. Posts are erected every three feet and the space between filled with a wattling of corn-stalks and sahuaro ribs. There is one entrance to the east. Then each village selects a definite part of the enclosure for its ceremonial objects and singers, the portion of the enclosure in the geographical direction of each village being selected by the members of that village. But nothing is put inside till the day of the feast and no markings are made or fences erected to designate the village selections.

In the center of this enclosure a post is then erected, about four feet in height. It is a log about six inches in thickness with a

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forked top, the post itself representing nothing. On the top is set a basket of corn-meal, on the east side of it is hung a representation of the sun and on the west side a representation of the moon. They are both made of cotton on a framework of sticks.

The representation of the sun (fig. 2 *a*) is flat, of a semi-oval shape and about three feet in length. A long twig is bent double in a horse-shoe-shape and the ends tied. A wattled framework is made by laying sticks over this close together and colored cotton is tied over both surfaces with string. The body is of white with a zigzag lightning band down the middle and at the lower edge is a wide

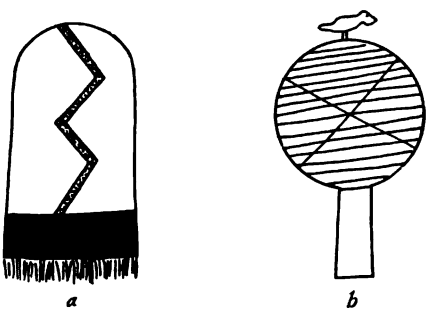



Fig. 2.—The representations of the sun and the moon, drawn from hasty sketches by natives.

black band with a hanging fringe. Around the entire edge are plumes of long feathers to represent the rays of the sun. In the fiesta it is carried by a man.

The symbol of the moon (fig. 2 *b*), on the other hand, is globular and hollow and is worn on the head of a man as a mask. It is about the same size as the sun and is also of varicolored cotton on a framework of twigs. Holes are left for eyes so that the one who wears it can see out. It has no feathers on it but a deer's tail is attached and it is surmounted by the figure of a small bird, painted black. In the fiesta the man who wears it wraps a mat around him and walks in a slow solemn manner.

On the morning of the tenth day, after the small enclosures are made, a little representation of a field-patch is made by each village near its enclosure. Sand is brought from the arroyo and placed

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near the field to represent the mouth of the arroyo close to the field. That evening before sunset all the clowns who have the proper costume (*nanawitcu*) dress up and go to see if the fields are in proper condition. They set to work and clean the ditches and the fields and burn the weeds, imitating people working in fields.

The following day the Vigita is celebrated. The *tcu'i'wa'tam* go around scattering flour on all those gathered, to keep sickness away from them, and as each person has the flour sprinkled on him he goes down to the dance-ground where the feast is to be held. All through the ten days' preparation only the men have worked but now the entire population takes part. As they near the place there is a small pile of prayer-sticks and every adult man takes one and cleanses himself with it, putting a little corn-meal on the stick and brushing it over himself. There are only a few sticks there and as each man cleanses himself he returns the stick to the pile. Meanwhile, at noon, the *nanawitcu* have been again to make sure that their fields are in perfect condition. Returning, they join their village groups and each group proceeds to the celebration ground. Every man in each village has busied himself making a representation of some object out of twigs and colored cotton and he carries this to the scene. These figures are of sahuaros, *tiorimus* (choyas), mezquite beans, *parmitas* (a small seed), clouds, deer or any object of ceremonial import. As the parade nears the ceremonial enclosure they pause, all sounding their bull-roarers, *wewegita*, which represent the sound of the rain.

All the men, the *vi'pinyim* or singers, the *nanawitcu* or clowns, and the unassigned men bearing the ceremonial objects enter the enclosure and deposit their paraphernalia in regular order, the women and children camping outside. The order observed is Achi, Kokmatkek, Santa Rosa, Akchin and Anekam. As each enters and deposits its gifts, the members run back towards the entrance swinging their bull-roarers to greet the next delegation. There is therefore a loud continuous roar like that of a rain-storm. When all have entered and grouped themselves in their respective places and deposited there their ceremonial objects, the *vipinyim* begin to sing. At first all sing together for a short while. Then,

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a little after dark, one of the best informed old men dresses up as a *nawitcu* and makes a set speech. At certain places in the speech, all the people howl like dogs or cackle like chickens or imitate any other animal except the coyote and owl.

After this speech the real singing begins. This is done by the *vi'pinyiм* from each village in the regular order, beginning with Achi. The singing continues all night, each village singing its eight songs and repeating as often as necessary. In each group there are two men known as *kokcɛpa* who represent the yellow corn. They wear masks which are painted blue on one side and yellow on the other, much like the masks of the *nanawitcu*, but with a long nose four inches in length and one small feather on the top of the head. After the *vi'pinyiм* have finished a song these shout "*kuh!*"

Just before dawn the *vi'pinyiм* cease singing and take off their clothes and paint themselves in a spotted fashion to represent the multicolored ears of corn, principally blue, red, and white. This is done in order to have plenty of crops. Meanwhile the men who are to bear the representations of the sun and the moon don their regalia and take up their burdens. Just at sunrise everything is finished and as the first rays of the sun appear the image of the sun passes out through the entrance, followed by that of the moon.

A few steps outside the entrance are placed two boys and two girls who perform a sort of a dance without moving their feet from one spot and without singing. The girls carry blue and white ears of dry corn while one boy bears a short stick and the other boy a bow without arrows. These children are known as *ha'kiwa'taм* and represent the children who were sacrificed to stop the great inundation. Near them are a number of old men in a line with notched sticks resting on baskets; these are rasped while one old man sings and the children dance. The old men represent the people of that mythical period and the song they sing is supposed to be of that distant age. For a short time the representations of the sun and moon remain before the singing patriarchs and the dancing children and then the *tcuiwa'taм* come and sprinkle the children with corn-meal. The effigies are then returned to the enclosure.

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When the sun and moon have been returned to their places the display of the ceremonial objects begins. A course has been cleared around the village and each object is taken from its place in the enclosure, carried around once and displayed to all the population and then returned. As usual, the villages proceed in order, led by Achi. All during the parade the *vi'pinyiм* sing and the *nanawitcu* play the clown. As there are plenty of objects, each one is carried around only once. As soon as one body leaves the enclosure, another is waiting inside, both of them singing. This ceremony continues until noon.

At noon the *nanawitcu* stage a representation of making sahuaro wine; they bring great ollas and act as if drunk. Others impersonate shamans and try to cause rain or perform other amusing acts. After this is over the *vi'pinyiм* come out, bearing representations of clouds[1] and swinging bull-roarers. The clouds at present are made of white cloth but formerly were of cotton. They are carried by the *vi'pinyiм* while the *nanawitcu* assist in supporting them with their sticks and thus they are paraded around the village. Lunch is then brought.

After a little rest for lunch the ceremony of the morning is repeated, the rest of the ceremonial objects being brought out and paraded around, Achi leading. The same object is shown only once. All afternoon this continues. After all the objects have been shown each village sings four songs, said to be different from the original eight, and the fiesta is then over.

But after the formal ending each village sings one song in the enclosure and then all leave for their homes singing. A few are left behind to dismantle the enclosure and everything is carried home except the corn-stalks. The head men of the feast may eat nothing but *atole* for four days after the celebration, and not very much of that. They must also wash in cold water and not

[1] "I visited the practising enclosure, which is from forty to fifty feet square. A great accumulation of ceremonial things was left here from preceding feasts, conspicuous among which were some large, triangular frames to which wads of cotton were attached, symbolizing clouds. Bright blue, wooden swallows, fastened to the tops of sticks, also attracted my attention. They are carried by certain performers. The enclosure is called a váaki, as is also the medicine lodge." Lumholtz, p. 97.

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sleep with their wives. If one were to disobey this injunction his penis would fall off and other harm would befall him. There are, however, no regulations to be observed before the celebration.[1]

The following eight songs were taken down as typical *Vigita* songs. They were sung by Achi at the last celebration.

1. geNhu ŋe tcevaŋi cahkali wucanyi
Over there the clouds in a row come out.
gaMhuŋ itoinaŋe dam ane muvitci
Over there our field above, there with corners
ane wucanyi tuahi djuhku
there come out. Thunders, rains.

2. atci itoinaŋ mehk osekaitaŋ iuŋidjeh
Achi our field far off is heard to shake.
dama itonenami tcevahaŋi wucanyeh
Above shining clouds come out.
ioh toinaŋ djuhku hunyi wucaNYE
Here our field rains, corn springs up.

3. vavahki eḍa vadjuhku'
Big house within it rains.
daMhana tcevahaŋi wucanyeh
Up above clouds come out.
sapowekaki namenoahi djuhku'
Well hear that thundering, rains.

4. winyim itoinaŋe winyim itoinaŋe
Winyim our fields, Winyim our fields,
damaiŋe huNya pewuwahkime
on them corn springs up.
yatci toinaŋe yatci toinaŋe
Achi our fields, Achi our fields,
tamaiŋe hunya pewuahkime
on them corn comes out.
ak'tcin it.oinaŋe ak'tcin it.oinaŋe
Akchin our fields, Akchin our fields,
yanegam it.oinaŋe anegam oinaŋe
Anekam our fields, Anekam fields,

5. yahtci t.oinaŋe damain cuda'ki merikuhte mamasemel
Achi our field on water ran ran.
iakonyehite iotam vahcaN nawitcu huhunyi
Here look people! Yonder clown ears of corn
behkeme behkeme
bears away, bears away.

6. wewesi u'si wehtceh wewesi u'si wehtceh
All sticks are there. All sticks are there.

[1] "Ten days before the principal men begin the preparations, fasting at the same time and drinking water but once a day." Lumholtz, p. 96.

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we's ametcutca kakai pevaupanyime
All we stand up lay across.
wes amatcutca vaupah
All we stand up lay across.

7. muhkisi tcewana kahtce
Dying world here lay.
muhkisi tcewana kahtce
Dying world here lay.
daMhanai hunyi wuca
Above it corn comes out.
mudatatci kiohta
Bend stalks.
damhana wuca djuhku
Above comes out. Rains.

8. yalisi tcetceto'ki litoI vavu'ca
Little green Montezuma is coming out.
yalisi dodo haiyu hunyi vuca
Little white corn is coming out.
mumui tcewaη akenyapenyukena
Many clouds rain on me.
mumui tcewaη akenyapenyukena
Many clouds rain on me.
tcewaη iwucanye kenyapenyukua
Clouds come out, rain on me.

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CHICAGO.

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# A NEW APPROACH TO HISTORY

By A. A. GOLDENWEISER

IT may or it may not be accidental that the interest in social science has lately received a mighty impetus. Books and articles on social and political theory, on democracy, the individual and the group, the state, the crowd, come from the press in well-nigh endless succession. The time indeed seems eminently ripe to reconsider our ideas of society and their application to life, for history has run amuck, and unless man interferes before it is too late, we may yet have to face the task of rebuilding the whole of civilization from the bottom up. As it always happens in cases like this, the more practical and immediate demands of the hour reëcho in the more remote realms of scientific thought and speculation. Thus the relations of history and ethnology to other sciences, such as psychology and sociology, have recently been reconsidered by Lowie,[1] Hocart,[2] Wissler,[3] and Rivers.[4] Kroeber has turned his attention to the theoretical relation of the historic to the biological sciences.[5] Going still further, the same writer published a somewhat cryptic, but none the less interesting, catechism of historic theory and methodology,[6] which elicited a spirited reply from Haeberlin.[7] Again, the danger of over-emphasizing the purely conventional barrier between the different social sciences was pointed out by the present writer, and this was made the point of

[1] "Psychology and Sociology," *American Journal of Sociology*, vol. XXI (1915), pp. 217–229.

[2] "Ethnology and Psychology," *Folklore*, vol. LXXV (1915), pp. 115–138.

[3] "Psychological and Historical Interpretations for Culture," *Science*, vol. XLIII (1916), pp. 193–201.

[4] "Sociology and Psychology," *The Sociological Review*, vol. IX (1916), pp. 1–13.

[5] "The Superorganic," *American Anthropologist*, vol. XIX (1917), pp. 163–213. See also the discussions by Sapir (*ibid.*, pp. 441–447) and Goldenweiser (*ibid.*, pp. 447–449).

[6] "The Eighteen Professions," *American Anthropologist*, vol. XIX (1917), pp. 283–289.

[7] "Anti-professions," *American Anthropologist, ibid.*, pp. 756–759.



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departure for a general theoretical analysis of the elements of history and culture.[1] To those interested in this once much cultivated but later somewhat neglected field, Professor Teggart's recent little volume[2] comes as a welcome contribution indeed. In more than one way the essay is timely and significant, while its contents will arouse in the mind of the student of culture (from an ethnological angle) frequent approval as well as equally emphatic disagreement. What the author purports to do—and of that larger endeavor the tiny volume before us is but a modest precursor —is to demonstrate "what sort of results might be obtained by a strict application of the method of science to the facts of history" (p. v). From another standpoint, the greater work will be "an attempt to do for history what biologists are engaged in doing for the history of the forms of life" (*ibid*).

In the section on "The Nature and Scope of the Inquiry" we are informed that

> Science is, fundamentally, a method of dealing with problems, and the initial step in any scientific undertaking is the determination of the problem to be investigated (p. 1).

The problem, then, in this humanistic inquiry is to ascertain "how man everywhere has come to be as he is" (p. 5). This formulation becomes the author's *Leitmotiv*, and thus we find it repeated many times in the course of the discussion (*e.g.*, on pp. 18, 25, 38, 90). Without much difficulty, in crisp and perfectly convincing statements, the author disposes of the physical, psychological, and climatic or environmental hypotheses, which have at various times

[1] "History, Psychology and Culture; Some Categories for an Introduction to Social Science," *Journal of Philosophy, Psychology and Scientific Methods*, vol. XV (October 10 and 22, 1918). See discussions by: Charles A. Ellwood, *ibid.*, January 30, 1918; Frederick J. Teggart, *ibid.*, March 13, 1918; H. D. Sheldon, *ibid.*, July 5, 1918. A. L. Kroeber's article, "The Possibility of a Social Psychology," *American Journal of Sociology*, vol. XXIII (1918), pp. 633–651; and H. K. Haeberlin's acute analysis of Wundt, "The Theoretical Foundations of Wundt's Volk-Psychology," *The Psychological Review*, vol. XXIII (1916), pp. 279–302, should be consulted in this connection.

[2] *The Processes of History*. By Frederick J. Teggart, Yale University, New Haven (1918), pp. IX + 162. See the same author's "Prolegomena to History," *University of California Publications in History*, vol. IV, no. 3 (Berkeley, 1916), and A. L. Kroeber's discussions (cited above.).

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been advanced to account for the differences of the various types of man "as he has come to be."[1] The upshot of the author's critique is that

> it has not seemed necessary to the exponents of these views to show how the factors described could have produced the differences which we see around us (p. 11).

In view of the tenacity with which the so-called economic interpretation of history still possesses the mind of man (as he has come to be), the author's censure on Marx's doctrine is particularly welcome; "He [Marx] neither considered the entire field of economic activity in modern life," writes Dr. Teggart, "nor the conditions of labor in any other than the capitalistic form of society;" and again: "this theory . . . is based upon a limited view of the facts, and represents the projection of a single factor upon the complexity of human experience" (pp. 16–17). Follows a brief discussion and critique of the concept of progress, which is as unusual as it is just, leading up to this categorical statement:

> If we look a little further, it will be to discover that human history is not unitary, but pluralistic; that what we are given is not one history, but many; and, that the concept of 'progress' is arrived at by the maintenance of a Europocentric tradition and the elimination from consideration of the activities of all peoples whose civilization does not at once appear as contributory to our own (p. 24).

Rather than

> to create narratives based upon the selection of events which seem to us of importance in view of some unverified theory of progress,

the author recommends that we

> compare these several histories [of different peoples] with the object of ascertaining what it is they hold in common (p. 25).

From this point on to the end of the first chapter the discussion takes us right to the kernel of the author's conception. Human history is here put on a level with

> other fields of history, such as astronomy, geology and biology (p. 26), [for] it comes to be seen that historical method is the same whatever the history investigated—whether that of the stellar universe, of the earth, of the forms of life upon the earth, or of man (p. 33).

[1] It must all along be remembered that "man" throughout this discussion often stands for "civilization," in its material as well as psychic manifestations.

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Moreover, the student of human history has a marked advantage over the historian of nature in so far as the former's record is definitely (or relatively definitely) chronologized.

It seems time to pause here, as the formulations toward the end of the chapter contrast strongly with the professions in the opening pages and the preface. We were told that the method of science was to be applied to human history, but further reading shows that the "method of science" is to be the method of the historic branches of the natural sciences, astronomy, geology, biology; for it will be admitted that these sciences have also non-historical aspects, a statement to a degree applicable also to the sciences of society. Nor is this all. The terms "scientific" and "scientific method" have acquired some of their most current connotations from their association with the so-called exact sciences, such as physics, chemistry, mathematics, or that mathematical branch of astronomy known as celestial mechanics. Scientific method in most general terms has thus come to mean one of two things: either 1, problem-working-hypothesis-experimentation (under controlled conditions). Acceptance or rejection of hypothesis-theory (sometimes designated as "principle" or "law"); or 2, theoretical formulation of a scheme or system of magnitudes, forces and correlations which, when applied to the interpretation of a particular, more or less complex set of facts and relations, proves a means of simplification or at least of consistent statement (this latter method being used in such sciences as theoretical physics and in some branches of celestial mechanics). Now, while some advocates of eugenics have proposed and to a degree carried out experiments, somewhat after the nature of the first of the above methods, whereas the second has been weekly adumbrated in some of the hypothetical constructs of modern ethnological diffusionists, all in all, there is no room in social science for either of the two characteristic "methods of science." However that may be, these methods are obviously out of court when one deals with the historical aspects of society or with the historical branches of such natural sciences as astronomy, geology or biology. If one further inquires for the particular method of the natural-historical sciences which the author would attempt to

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emulate in his study, it readily appears that what he essays is the *determination of constants*. In this endeavor he will have with him all those whose minds are wont to be perturbed by the contemplation of the immensely complicated and apparently disorderly successions of historic happenings. But here there arises one further query. Suppose such a constant, nay, a set of constants, were disclosed, thus greatly enhancing our insight into historical processes. Still, from what we know of history and of man (as he has come to be), it is but reasonable to expect that these constants would not prove a complete rationale of history, but of certain more or less prominent aspects of it. Now, in the facts and successions constituting the subject-matter of the natural-historical sciences there also are discernible certain constants as well as certain variants, but it so happens that the variants, in these cases, do not interest us, or interest us but slightly; fortunately so, for, as the author notes, we lack the means of reconstructing the minutiæ of these processes and of chronologizing them. Not so in history. Without attempting to raise from well-deserved historical obscurity the proverbial death of a neighbor's cat, it is but fair to doubt whether the historic constants—when disclosed—will cover all that is theoretically interesting and humanly significant. To disregard such residual facts and successions would be to sacrifice reality to method, to accept them, on the other hand, would mean to assign to the results of the method a relatively modest place as heuristic tools in historic study.

We may now proceed to an examination of the author's constants. The second chapter on "The Geographical Factor in History" is devoted to a demonstration of what Professor Teggart calls "the homogeneity of history." The thesis in the author's own words is as follows:

> The fundamental basis of argument for holding that the history of man everywhere is of the same fabric, does not rest upon the inter-connection of events, but may be stated in the form that the varying experiences of human groups have been similiarly conditioned by the varying aspects of the conformation of the globe. Man cannot escape the physical world in which he lives, nor its infinite diversification; this is obvious, but it will require some illustration to make clear the fact that the even-handed dominance of nature leads inevitably to widely different results in the lives of men (pp. 44–5).

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Now, this

close dependence of history upon the irregularities of the surface of the earth (p. 47) [is exemplified in] another aspect of homogeneity, which is, that the political organizations dealt with in history have all come into being at definite and restricted spots, from which, subsequently they have expanded (p. 48).

It will thus be seen that the inquiry is henceforth limited "to the beginnings of political organization" (p. 49). Furthermore,

this determinant influence of routes has been dependent upon the presence of human beings, . . . it comes into play only in case of the movement of peoples [migrations]. Hence the origin of these movements becomes a matter of primary importance, more particularly as the homogeneity of history is further exhibited in the dependence of these movements of migrations upon man's physical surroundings (pp. 52–3).

The environmental feature responsible for such migrations finally is shown to consist in destructive changes of climate (pp. 68–75). The cycle of constants thus brought to light may now be schematized as follows: pressure of deficient food supply brought about through destructive changes of climate; migration; friction with preëxisting populations at a geographically conditioned terminus of the route of travel taken by the migration; emergence of political organization. This successive series is again resolvable into two causal constants: 1, the relation of migration to certain adverse climatic conditions; and 2, the relation of political organization to migration plus certain specific geographical conditions.

In examining point (1)[1] it may be admitted from the outset

[1] Two subsidiary points in the author's argument must be met here. The first refers to the balance of population in primitive conditions which, according to the author, "in normal stable conditions remains stationary; . . . among primitive peoples there is no "national increase" which would lead inevitably to migrations" (p. 64). In this connection the prevalence and importance of infanticide is emphasized. Now, while it will be time enough to examine the author's evidence when it is produced, it may not be amiss to state right here that Professor Teggart's assertion that "infanticide, the killing of new-born infants, has been practised universally throughout the world (p. 58) is not supported by ethnological experience, within the knowledge of the reviewer. As to the alleged absence of the "natural increase" in primitive populations, it is worth notice that whereas in aboriginal Australia, the South Sea area, and the two Americas the density of population has for exceedingly long periods remained on a level incomparably below modern standards, the population of primitive Africa has increased to a degree which, barring the populations of modern cities, may well stand comparison with many areas in civilization. The author's second point refers to

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that changes of climate destructive enough to make the discomforts of mass migration preferable to readjustment *in loco*, would be likely to result in such migration. That precisely such climatic cataclisms will result in the destruction and mass migration of animals, has often been hypothesized and described. Kropotkin has given us a very vivid picture of these phenomena in the opening chapters of his "Mutual Aid." After Pumpelly's expedition, to which Professor Teggart gives due credit, there remains no doubt that periods of desiccation in Turkestan were accompanied by migrations, evidently on a considerable scale. All this however is not sufficient to justify the designation of a "constant" in application to the causal link "climate-migration." For, does violent climatic change always cause migration and does migration follow from no other antecedents? I am unable to answer the first query without further consultation of relevant data, should such be in existence. As to the second, it must be answered in the negative in the face of those vast areas of human migrations to which reference was made above. That climatic changes should have been responsible, for instance, for the migrations of hordes of Athapascans from the interior of Canada along the Pacific coast and down to the Pueblos of the Southwest, of this there is, to my knowledge, no evidence whatsoever. The same applies to the Bantu migrations of the southeast of Africa. As to the migrations of the Papuans and Melanesians, what we know of their direction and extent discourages any climatological interpretation. Moreover, the

the unlikelihood of migration on anything like a large scale unless the people are actually "driven"; for we learn that "man is prone to remain where he is, to fixity in ideas and in ways of doing things, and only through nature's insistant driving has he been shaken out of his immobility and set wayfaring upon the open road" (p. 76). To this is joined the somewhat absurd assertion of Keane that "most African negroes south of the equator, most Oceanic negroes (Melanesians and Papuans), all Australian and American aborigines have remained in their original habitats ever since what may be called the first settlement of the earth by man [*sic* !]" (p. 64). Without disputing in the least the faith in human inertia reflected in the above general statement, and siding with the author in his rejection of the hypothesis that "man is primarily a migratory, restless being "(p. 76), one is but little impressed by specifications such as these in the face of the extant evidence for minor as well as major migrations provided by the linguistic map of North America, by traditional and much convergent semi-historical material from the entire southeast of Africa, and by traditional, somatological and general cultural data from the Papuan-Melansian district.

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latter district is flanked by two nuclei of migrations, the Malay and the Polynesian, which in extent and, in case of the latter, also in complexity, can scarcely be matched by any other migrations in human history. That primitive peoples, navigating what after all were crude and flimsy vessels, should have succeeded in linking by a continuous chain of migrations the shores of Easter island with those of Madagascar, must be pronounced as truly remarkable; and it seems obvious that changes of climate had nothing whatever to do with these movements. Also, if it is objected that the numbers involved at any given time were small, one may well reply that the means of transportation available precluded the simultaneous movement of larger numbers, that, in proportion to the probable density of populations among those peoples, the few were not so few, and, finally, that where the few moved the many might have moved also.

Before the causal link "migration-political organization" can be discussed, we must turn to the opening paragraph of the chapter on "The Human Factor in History," in which the author's conception of political organization receives more precise formulation. "Political organization is a comparatively recent phenomenon" . . . (p. 79) reads the first sentence. Again: "Political organization is an exceptional thing, characteristic only of certain groups" (*ibid.*). Now, strictly speaking, statements such as these must be declared wholly erroneous. For political organization, as an expression of the integrating tendency in society, as contrasted with the differentiating tendency finding expression in social organization (in the narrower sense), is as old as the latter, and, in a sense, as old as society itself. As far as the student's eye can reach, it seems, man recognized, unconsciously though it may have been, the sovereignty of the group, speaking a common language (or dialect), occupying a more or less definitely circumscribed territory, having within that territory certain privileges (denied to outsiders) and sharing together certain locally particularized customs and traditions. This sovereignty often expresses itself in the prestige and influence wielded by the tribal old men, or by a chief or chiefs. In later periods the territorial expanse, the numerical strength and



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the functional integration of the tribe or tribal cluster, conspicuously increase. It is true that some of the functions inherent in the modern state, such as the more narrowly administrative ones, or, in recent times, economic ones, are but weakly represented in those primitive political aggregates, if indeed they are represented at all. This however, is a question of a different order; the political organization itself, being, it seems, a quasi-organic attribute of human groups (for is not man a *ξῶον πολιτικόν*?) is there nevertheless. It is, therefore, also erroneous to contrast, as does the author (following though he does authoritative precedent), "primitive" and "civilized" groups of men by stating that

> among the former, the individual identifies himself by particularizing his blood relationship, whereas, in the latter, the individual defines his status in terms of relation to a given territory (p. 80).

The territorial organization of the state, with its manifold functions, does stand out as something in its entirety foreign to primitive society, but the contrast is mitigated when comparison is made not with the kinship grouping, representing a social principle on a different level, but with tribal political organization, finding expression in territory, language, custom, chieftainship, etc. The kinship organization of primitive society, on the other hand, together with its organization into families, occasionally overshadowed though the latter may be by the former, should properly be juxtaposed to the modern family organization. For do not family and clan (or gens) both represent a kinship grouping based on blood relationship, actual in the family, often assumed in the clan (or gens)? The family, moreover, recognizes simultaneously both lines of descent (paternal and maternal), whereas but one is considered in the clan (maternal) and gens (paternal). If comparison is made on this basis, it will, I think, be found, that the family is perhaps no less important in the modern territorial state than the clan (gens) and sometimes family are in the primitive political unit (tribe or tribal cluster); and in both instances, some of the traits and functions of these kinship groups are contingent upon their inclusion in the political unit,[1] while they also possess

[1] A possible objection might be raised to the preceding argument on the ground that the term "political organization" has in it been given a different connotation from

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other traits, and functions inherent in their character of kin (or blood) groups.

If, then, it is accepted that political organization is inherent in society, migration evidently has nothing to do with it. In fairness to the author, however, let us glance for a moment at forms of political organization coming nearer to those with which he would specifically deal. The reference is to political systems of a higher degree of integration and centralization than is common in most primitive society. Such political systems occur in three wide geographical areas: parts of North America, a large part of Bantu and Sudanese Africa, and Polynesia. The political form which is indigenous in North America (although it occurs in a limited number of instances only) is the Confederacy, that of the Iroquois being best known. It involves, of course, a considerable degree of integration of the functions of the constituent tribes, but lacks the feature of supreme authority being lodged in one ruling head, the administrative authority and functions of the body of semi-elected, semi-hereditary chiefs also being distinctly limited. The African state approaches the Eurasian form much more closely, in so far as the territory occupied is often considerable, the individuals comprised in the state number hundreds of thousands or even millions, the centralization of administrative functions is marked, and the state is headed by a king, hereditary, of sacred person, owner of the state land, absolute master over the life and death of his subjects, and legislator. In Polynesia, the territorial and population proportions are again reduced to the more primitive level, but there is a king, whose person is sacred, whose prestige is tremendous, who, without being a legislator, wields the almost equivalent power of the imposition of *tabu*. Should one look about for

that adhered to by the author. It is true that Professor Teggart is at liberty to use terms with whatever meaning may to him seem appropriate, provided the use is consistent; but the issue here is not terminological, for when it is possible to show, as in the above excursion, that certain features of political organization in its modern sense (meaning the "State" of history) are shared by human aggregates down to remotest antiquity, the "emergence of political organization" (in the modern sense) does no longer appear as so much of an epochal event in world history, and the "processes" that would account for its emergence must share with it this change of perspective.

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any possible connection of these political structures with migrations, the following results would appear: in America, the tribes of the Iroquois are known to have resided in their approximate locations at the formation of the League long before that event occurred; also, a series of migrations in that continent, some of vast extent (as noted before) did not result in the germination of political structures on a larger scale. In Africa, while the southeastern area of migration, mentioned above, coincides with the presence of state systems such as that of the Zulu Kaffirs, no conspicuous migrations have been recorded in the other much larger sections of the aboriginal continent, where similar states are equally common. In Polynesia, finally, there is correspondence between integrated political structures and an area of vast migrations, but next door to that area, in the Papuan-Melanesian district, not inconsiderable migrations have not led to a similar political result.

In the light of the above considerations, one will, I trust, hesitate in ascribing determining value to migrations, as such, in relation to political organization, as such, even should Professor Teggart succeed in showing that there exists a fairly constant nexus between a certain type of migrations and the emergence of modern or more strictly historical states.

Before proceeding, there is another point made by the author with reference to the relation of kindred to political organization, which must be met here. The statement runs as follows:

> To comprehend the situation fully, we may begin by saying that kindred organization, in whatever form it may assume, reflects the natural facts of human generation. What follows immediately from this is a commonplace of the study of primitive man which must be constantly borne in mind, for kindred organization implies the unquestioned and unremitting dominance of the group over the individual, and this leads to the tenacious and uncompromising maintenance of customary ways and ideas. It will thus be seen that the despotism of custom negatives the idea that kindred organization could have been given up voluntarily, or exchanged, after deliberation, for something invented or considered better. The change, as I have pointed out, has been forced upon men at certain geographical points, determined by the physical distribution of land and water, and by a series of exigencies which go back to specific changes in climate within a definite area of the earth's surface. Furthermore, the immediate occasion of the break-up of kindred groups has been the collision and conflict, at the termini of routes, which have ensued from the migrations of men. . . . (pp. 84–5).

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This conception is perhaps the most surprising and least acceptable in the entire volume. There is something quaintly humorous in the idea of these magical geographical termini at which, again and again, time-worn traditions and customs, including kindred organization itself, are shed like old scales, to make room for individualism, political organization and the modern world. One fears that the "migration-political organization" constant might turn top-heavy, if to its other burdens is added this one of the breakdown of kinship; for history, after all, is not wont to indulge in anything quite so alluringly romantic, and, if she does, she does not repeat herself.

The three primitive areas mentioned before where political organization has reached its culmination points, may incidentally serve as illustrations of possible types of historic relation between social and political units. Among the Iroquois, the political system has emerged through a double integration, of tribal units, on the one hand, and of clans, on the other. In this process, while the tribal units have lost a great deal of their former independence, the clan units do not seem to have been similarly affected, remaining in full possession of multifarious functions, notwithstanding the extension of some of the latter to homologous clans throughout the League. In Africa, the socio-political systems present the curious picture of mostly gentile organizations of obviously great antiquity overlaid by political structures of more recent origin. The gentes, moreover, have undergone mutations in various ways; thus, the numbers of individuals constituting a gens, originally no doubt limited to proportions compatible with a society based on genuine kinship grouping, have grown so large, extending often into many thousands, that the kinship character of the gentes has of necessity become very much attenuated; again, many gentes have assumed functions associated with the requirements of the political system, functions which originally must have been foreign to these units. The kinship organizations of Africa are evidently on the way of passing into something which is no longer a kinship organization. In Polynesia, finally, the clan basis of society, of which the traces are obvious enough, has almost wholly ceased to exist, ceding its

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place to local units which here constitute the minor divisions of the political aggregate. Turning, finally, to the political organizations with which Professor Teggart is primarily concerned, the demonstration has never been made, if it ever will be, that all the peoples of ancient Eurasia were once divided into clans or gentes. Some of them, no doubt, were; but it is highly improbable that many of these should have preserved their kinship systems up to the late time of the inception of the great historic states. Most of these systems must have passed out long before, nor is the only possible alternative to be found in the assumption that they "could have been given up voluntarily or exchanged, after deliberation" (an assumption we might leave for J. G. Frazer to defend against the author).

The remaining part of the chapter on "The Human Factor" contains some of the most suggestive ideas of the book. One feels that in a future elaboration of his study, the author will be able to make an impressive case for his position with reference to the cultural significance of the detached individual and the nature and behavior of idea-systems. Some brief comments, at this time, will, however, not be amiss. Returning once more to those conflicts at the terminal points of migrations with which we are already familiar, the author proceeds:

> The cardinal point is that the conflict, in breaking up the older organization, liberated the individual man, if but for a moment, from the dominance of the group, its observances, its formulae, and its ideas. Briefly, a situation was created in which the old rites and ceremonies could not be performed, one in which the old rules of action were manifestly inadequate, and hence one in which the individual became, in some measure, a law unto himself. This, at bottom, is the fact upon which all history turns (p. 84).

And again:

> Most significant of all, the central feature of transition is not merely the substitution of territory for blood relationship as the basis of unity in human groups, but the emergence of individuality and of personal self-assertion, and hence it follows that human advance rests ultimately upon the foundation of individual initiative and activity (p. 98).

As contrasted with this, the conditions in primitive society are sketched in the following words:

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So completely was the individual subordinated to the community that art was just the repetition of tribal designs, literature the repetition of tribal songs, and religion the repetition of tribal rites (pp. 86–7).

And, once more:

the traditional ideas entertained have, in general, been transmutted into customary actions and ways of doing things. So, religious ideas are concentrated in rites and observances, and explanations of natural phenomena are embodied in symbolic ceremonies. In short, the whole body of custom and tradition represents ideas fixed in action. Since these modes of action, which are associated with the essential activities of life, must be prosecuted with rigid adherence to precedent, it is evident that any reconsideration ot the validity of the ideas upon which they rest is practically out of the question. Primitive man does not "think," he performs definitely prescribed actions under the eye of the community, which, in turn, is vitally concerned in the exactness with which the repetition of formula or ceremony is carried out (p. 108).

It would be futile to dispute the profound significance for progress of the free creative individual, as attested to in the above passages. Also, the contrast, from this standpoint, between modern and primitive conditions, is, in the main, indubitably correct. But when we are asked to look upon the terminal points of migrations with their cultural conflicts, as the birthplaces of individualism, agreement must be withheld. However tradition-ridden primitive society may be, it is far from presenting the characteristics of a well-nigh automatic perpetuator of traditional usage with which the author attempts to endow it. A superficial survey of primitive art, religion, and mythology would suffice to show that oversocialized though primitive man may be, he does not merely draw to pattern, repeat by rote ancestral stories and mutter timeworn incantations. While the range of individual creativeness is certainly limited, when compared with modern standards, while the self-consciousness of the primitive artist may impress us as insignificant, there is creativeness, in art, religion, myth-telling and myth-making, in which man and woman participate. That primitive man does not think is as little true as the obsolete dogmas that he has no power of abstraction or that his language has no grammar. It must also be remembered that Professor Teggart, like so many before him, portrays primitive man as if he were free from those matter-of-fact activities, which, in fact, constitute to

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him, as they do to us, the very core of the struggle of life. Apart from song and dance, from prayer, incantation and sacred rite, from myth-telling and listening, from painting, carving and embroidering, there goes on from day to day the serious and hard business of hunting and fishing, the building of traps and snares, of houses and canoes, the making of pots and the weaving of baskets, spinning and sewing, the making of weapons and the using of them; all this serious and hard business, the descriptions of which fill large sections of our ethnological monographs, is carried on from day to day, by man and by woman, in complete oblivion of the supernatural and frequent disregard of the esthetic, with the senses pitched high, and the mind alert, observing, trying, improving, inventing, achieving expertness and success. While all this is done within the more or less narrow bounds of accepted use and wont, traditionally derived and socially imposed, it is the individual who does the work, who adjusts himself, who creates. Thus, whatever factors may be held responsible for the precipitation at certain times and places of individual detachment, self-assertion and originality, it is, after all, but a precipitation of certain qualities of the individual which have asserted themselves all along and have left their mark on the many types of civilization encompassed in the primitive world.

Note must be taken here of another conception which, together with the emergence of individuality, is emphasized in this section of the work; a conception which is not new, but appears, in the author's hands, in a somewhat novel illumination. The conception is that of an idea-system as characterizing a particular form of civilization, at a given place and time. The following passages may serve to elucidate the author's point:

> If then, we come to compare, not man and brute, but the differing groups that go to make up the human population of the globe, the distinguishing feature of any group will be, not its language, implements, or institutions, but its particular idea-system, of which these other manifestations of activity are varying expressions. Without exception the products of human activity are expressions or aspects of the entire mental content of the group or individual. This mental content, moreover, is not to be conceived of as a mere assemblage of disparate units placed in juxtaposition, but as cohering in an idea-system. Ideas are not simply accumulated or heaped up; on the contrary, every "new" idea added

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not only modifies, but is in turn modified by the existing system into which it is incorporated (pp. 102–3).

And again:

It will appear, then, that if we are to consider the content of life in addition to the exterior forms of human association, the study before us must concern itself with the factors and processes through which the idea-systems of different groups have come to be as we find them today (p. 103).

Now, all this is very suggestive, but also very unclear. A good many interpretations could be given of the author's formulation; thus, it will be best to defer more deliberate discussion until concrete performance has clarified the author's intention. One or two points, however, should not be passed over in silence. We are told that "language, implements and institutions" are expressions of an idea-system. Two questions are in order here: in what sense are they expressions? and are they expressions of an idea-system? It is well understood that language is an expression of thought, but also determines thought; that implements are outgrowths of tasks to be achieved, but also determine or modify such tasks; that institutions spring from certain tendencies, attitudes and needs, but, once more, are moulders as well as moulded. In other words, the psychological or psycho-sociological requirements, which may be posited as the primary factor, presently receive concrete embodiment in act, tool, or code, which henceforth are operative in producing shifts in the original psychological factors and in creating new ones with which the process starts anew. Thus the objective and behavioristic elements of a civilization can never be regarded as direct expressions of the ideas or idea-systems that have, or may have, originally engendered them, but are, in fact, indefinitely and often irredeemably removed from them. Neither the objective nor the psychological factors can in this context be regarded as either wholly passive or wholly active. There is rather a continuous give and take. Only in one sense, moreover, may one speak of one idea-system as underlying a state of civilization—and this brings us to our second question—in the sense, namely, that every civilization displays to a greater or less extent the oft-recognized tendency of integrating and assimilating, psychologically, the

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heterogeneous and variously derived elements of which it is composed. Therefore, I suspect, it is more or less coördinated clusters of ideas or idea-systems rather than singular idea-systems that the author will encounter in his comparative study of different forms and states of civilization. On the other hand, the author is to be congratulated upon his endorsement of the view, latterly decidedly on the ascendent, that "human advancement is the outcome of the commingling of ideas through the contact of different groups" (p. 111). It is to be expected that the evidently partial truth of this statement will be done justice to in the author's fuller treatment of the subject; also, considering that the theoretical postulate of progress through "the commingling of ideas" has, in the domain of ethnology, led to such widely different systems as those of Graebner, Rivers, and Boas, it is not a little curious in what particular way the author will handle the difficult set of problems involved.[1]

One has a sharp sense of disappointment to find that, in the last analysis, the subject of idea-systems is brought down to the wholly inadequate level of the environmentalist. We read: "Differences in idea-systems are, fundamentally, man's response to differences in his surroundings" (p. 113, and similar statements on pp. 117 and 118). Must we hear once more that the "surroundings" of a group "determine" its primary interests and that these, the primary economic interests, determine the system of ideas? What the "surroundings" do effect is, at most, to hold man to an adjustment once made (the adjustment itself always remaining one of a set of possible ones), also offering considerable resistance to further readjustments.[2] As to idea-systems, when we hear that while the language of the Eskimo has many different words for "seal," that of the Arab displays a similar elaboration of terms for the "camel" (p. 114), the cultural significance of this is by no means apparent. In fact, both might conceivably have similar or identical

[1] *Cf.* also my "History, Psychology and Culture," etc., *Journal of Philosophy, Psychology and Scientific Methods*, vol. XV (1918), in which a number of points bearing on the theoretical issues mentioned above are discussed under the general caption of "accidental" factors.

[2] See Clark Wissler, "Aboriginal Maize Culture as a Typical Culture Complex," *American Journal of Sociology*, vol. XXI, 1916.

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idea-systems, the camel with its terminology fitting into the same pigeonhole, in one case, as the seal with its terminological derivatives does in the other. Nor is it at all clear why fishing, cattle-raising and farming should determine different idea-systems, except in a sense much more restricted than that implied in the author's formulation. Instances substantiating this statement are as familiar to the author as they are to the present writer.

The author's argument in the last chapter on "Methods and Results" is, perhaps, less striking and daring, but his reasoning is in closer touch with concrete fact and his analysis is often impressive. Turning to a general characterization of the nature and mechanisms of civilization, the author asks, with Bagehot: "If fixity is an invariable ingredient in early civilization, how then did any civilization become unfixed?" (pp. 130–1). The task then is, first, to disclose the processes which work for stagnation, then those that bring about change. For purposes of such an analysis the author takes man for granted, leaving the problem of his physical history to the biologist, and also postulates a general psychological comparability of mankind. That the latter postulate is but a working hypothesis, of this the author is well aware; it is for him a "methodological assumption set up for purposes of a particular investigation" (p. 136). The pages devoted to an analysis of the working of social inertia, conservatism, tradition, will stand careful perusal (pp. 138–140). They close with this pertinent and timely remark: "While, then, educative discipline tends to preserve what has been acquired, it presents a very real obstacle to further advance" (p. 140). Fixity, however, is not all, for were culture nothing but a method of preserving the past, progress would be impossible. "Under actual conditions," writes Dr. Teggart, "this fixity of ideas is never complete, and in all human groups there may be observed in operation certain processes through which idea-systems are being slowly but continuously modified" (p. 141). In dealing with these changes the author displays what seems exaggerated caution and fearfulness,—one perceives how deeply stirred his spirit must be by the unique significance of the history-moulding episodes at the termini of migrations. Scant justice is done, there-

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fore, to the civilizational contribution of the "great man" and even to that of the "contact of peoples," although the author ascribes not a little significance to this latter factor. We read:

> For more than one reason, indeed, no "genius" can make any great departure from the idea-system of his people; the individual may influence the group, but such modifications as he may succeed in introducing will proceed along established lines, and cannot be regarded as significant "changes" (p. 143).

This careless and unjust statement scarcely requires serious refutation. Again, with reference to the changes induced by the spread of cultural features, through the "contact of peoples," but unaccompanied by invasion *en masse* and conflict *in loco*, the author writes:

> The reason [for the inability of these factors to further "advance"] is not far to seek, for while the contact process may tend theoretically, to bring all groups to the level of the highest, it cannot serve to place any one group far in advance of the rest (p. 146).

While a book might, perhaps, be needed to disprove this assertion, it is worth pointing out that the principle of creative synthesis, so brilliantly formulated by Wundt, stands in direct opposition to the author's allegation. The setting free of reserve energies, the release of powers clogged up by traditional rut, under the provocation of apparently inconspicuous events introduced through foreign contact or inner changes, these well known and often described psycho-sociological phenomena guarantee the almost unlimited possibilities of the production of much from little in matters cultural. James's admirable remarks, quoted by the author with undisguised appreciation, bear directly on the issue at hand.

In conclusion I want to quote two statements which give a succinct summary of the author's position. Writes Dr. Teggart:

> What we find actually throughout the course of history are the unmistakable results of constant processes manifested in fixity or persistence, tempered by other processes which gradually effect a modification of this rigidity. In addition to these two sets of processes, however, there is abundant evidence of the fact that at different times and in different places certain events have led to significant changes in the groups affected, and that these changes stand in direct relation to the phenomenon of "advance" (p. 148).

And again:

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The hypothesis required may now be stated in the form that human advancement follows upon the mental release, of the members of a group or of a single individual, from the authority of an established system of ideas. This release has, in the past, been occasioned through the breaking down of previous idea-systems by prolonged struggles between opposing groups which have been brought into conflict as a result of the involuntary movements of peoples. What follows is the building up of a new idea-system, which is not a simple cumulation of the knowledge previously accepted, but the product of critical activity stirred by the perception of conflicting elements in the opposed idea-systems (pp. 151–2).

As one looks back, synthetically, at the author's effort, its timeliness and significance are strikingly revealed. The pressing into service of the different social sciences in the common enterprise of making clear the history of man, is a task of which the execution has recently been advocated from quite different quarters;[1] the theoretical importance, for certain purposes, of breaking through the accepted lines of demarcation between the conventionally recognized social sciences, has also been indicated.[2] The author is to be commended for his advocacy of more precise methodology in the solution of specific problems in historic research, to supplement, we hope, not to supplant, the less rigorous procedure of the more subjective type of interpretative historic narrative. In so far as the author's immediate endeavor will consist in the determination of historic constants, he will certainly enjoy the support and keenest interest of all students of man and his history. It may be doubted, however, whether any constants thus revealed will prove as categorical as those of some natural sciences, not to speak of those of the exact sciences. To all appearance, the author is free from all racial bias and accepts man's culture the world over as furnishing strictly comparable material for historic study. Certain more extreme forms of environmental and economic interpretation are vigorously attacked and rejected. The scope and perspective of historic study is deliberately pushed beyond the boundaries of

[1] *Cf.* Clark Wissler, "Historical and Psychological Interpretations in Culture," *Science*, vol. XLIII, 1916, and his "The American Indian", Introduction.

[2] *Cf.* A. A. Goldenweiser, "History, Psychology and Culture," etc., *Journal of Philosophy, Psychology and Scientific Methods*, vol. XV (1918), Reprint, pp. 1–2. From the standpoint of educational policy, the utilization of this principle will be found exemplified in the announcement of courses for 1919–1920 of the New School for Social Research, New York.

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Europe, to the inclusion, especially, of the great continental mass of Asia.[1] Again, the author's careful attention to the psychological factors involved in historic reconstruction, deserve especially warm support in these days of behaviorism and statistics. The approach of the mental side of human advance from the standpoint of the growth, conflict and transformation of idea-systems, holds out many alluring vistas, of interest and concern not alone to the historian and sociologist, but to the psychologist (barring the behaviorist faction) and the philosopher. General recognition must also be granted to the author's tripartite classification of the factors involved in the maintenance and growth of civilization, the factor of persistence and fixity, that of gradual cumulative change, and, finally, that of violent transformation leading to definite "advance." As has been seen, however, the scope given to the phenomenon of "advance" cannot be accepted without reservation.

On the other hand, the specific formulation of the author's task cannot be pronounced as entirely satisfactory. That his attempt represents an application of the method of science to the study of man, cannot be accepted, for, in the last analysis, what he wants to do is to lay bare certain constants in the determinamination of historic successions, an enterprise which theoretically lies in the level of the historic branches of the natural sciences such as astronomy, geology, and biology, but which must also be quite different, in its problems as well as its methods, from the procedures characteristic of the non-historic branches of the natural sciences as well as of the entire field of exact science. Again, the narrowing down of the investigation to the determinants of "political organization," while permissible in itself, seems to have impeded the author's recognition of the vast multiplicity of factors of historic causation. The connotation given to the term "political organization" is too narrow, an issue not merely of terminological character, for a wider view readily reveals the fact that some fundamental features of political organization are inherent in all

[1] It is worth noting that a similar extension of historic outlook on an even vaster scale, was theoretically advocated by Lamprecht, *Die Moderne Geschichtswissenschaft*, and that the task has been actually attacked by Breysig, *Die Geschichte der Menschheit;* the highly speculative and almost phantastic excursions of the latter author could, however, only serve to bring the entire scheme into undeserved disrepute.

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society, a discovery which cannot but change the perspective in which the processes which may, perhaps, be shown to be responsible for the emergence of the more modern type of political organization, will appear to the investigator. Further, the authors "constants," in so far as indicated in this preliminary study, are subject to criticism. The constant "climate-migration," while no doubt having a basis of fact, falls far short of representing a necessary or constant causal succession, for while it may be provisionally admitted that climatic changes of sufficient magnitude and destructiveness will probably always result in mass migration, migrations can also be shown to be due to a great variety of other factors. Again the "migration-political organization" constant, whatever the result of the author's attempt to demonstrate it for a particular type of political organization, amounts at best but to one of many factors involved in the process, for migrations not accompanied by the formation of political organization (even of the more centralized variety) are as common as political organizations the roots of which do not rest in migrations. Adverse climatic change—desiccation—migration *en masse*—conflicts at the terminal point of the route of travel—occupation of invaded territory—conflict of idea-systems—dissolution of established custom and belief—liberation of the individual—criticism—creativeness—advance, these constitute, Professor Teggart would have us believe, the "processes of history," and in their uniformity lies the "homogeneity "of history. We have seen how much truth there may lie in certain aspects of this complex; perhaps the author's subsequent demonstrations will enhance the probability of certain further parts of his theory, but in its entirety, as representing the "processes of history" and as proof of historic homogeneity, the theory must be rejected. It is, moreover, incumbent upon those who may see the author's contribution in the light of the present writer, to lay bare its failings before the interested students of society, for the theory, not unlike the anthropo-geographical ideas of Ratzel, is by no means devoid of those alluring features of simplicity, definiteness and grandioseness, to which the mind of man, ever eager for finality and repose, continues to fall an easy prey.

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# TOTEM AND TABOO: AN ETHNOLOGIC PSYCHOANALYSIS

By A. L. KROEBER

THE recent translation into English of Freud's interpretation of a number of ethnic phenomena[1] offers an occasion to review the startling series of essays which first appeared in *Imago* a number of years ago. There is the more reason for this because, little as this particular work of Freud has been noticed by anthropologists, the vogue of the psychoanalytic movement founded by him is now so strong that the book is certain to make an impression in many intelligent circles.

Freud's principal thesis emerges formally only toward the end of his book, but evidently has controlled his reasoning from the beginning, although perhaps unconsciously. This thesis is (p. 258) "that the beginnings of religion, ethics, society, and art meet in the Oedipus complex." He commences with the inference of Darwin, developed farther by Atkinson, that at a very early period man lived in small communities consisting of an adult male and a number of females and immature individuals, the males among the latter being driven off by the head of the group as they became old enough to evoke his jealousy. To this Freud adds the Robertson Smith theory that sacrifice at the altar is the essential element in every ancient cult, and that such sacrifice goes back to a killing and eating by the clan of its totem animal, which was regarded as of kin with the clan and its god, and whose killing at ordinary times was therefore strictly forbidden. The Oedipus complex directed upon these two hypotheses welds them into a mechanism with which it is possible to explain most of the essentials of human civilization, as follows. The expelled sons of the primal horde finally banded

[1] *Totem and Taboo: Resemblances between the Psychic Life of Savages and Neurotics*, by Sigmund Freud. Authorized English Translation, with Introduction, by A. A. Brill. New York, Moffat Yard & Co., 1918.



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together and slew their father, ate him, and appropriated the females. In this they satisfied the same hate impulse that is a normal infantile trait and the basis of most neuroses, but which often leads to unconscious "displacement" of feelings, especially upon animals. At this point, however, the ambivalence of emotions proved decisive. The tender feelings which had always persisted by the side of the brothers' hate for their father, gained the upper hand as soon as this hate was satisfied, and took the form of remorse and sense of guilt. "What the father's presence had formerly prevented they themselves now prohibited in the psychic situation of 'subsequent obedience' which we know so well from psychoanalysis. They undid their deed by declaring that the killing of the father substitute, the totem, was not allowed, and renounced the fruits of their deed by denying themselves the liberated women. Thus they created the two fundamental taboos of totemism" (p. 236). These are "the oldest and most important taboos" of mankind: "namely not to kill the totem animal and to avoid sexual intercourse with totem companions of the other sex" (p. 53), alongside which many if not all other taboos are "secondary, displaced and distorted." The renunciation of the women or incest prohibition had also this practical foundation: that any attempt to divide the spoils, when each member of the band really wished to emulate the father and possess all the women, would have disrupted the organization which had made the brothers strong (p. 237). The totem sacrifice and feast reflected the killing and eating of the father, assuaged "the burning sense of guilt," and brought about "a kind of reconciliation" or agreement by which the father-totem granted all wishes of his sons in return for their pledge to honor his life (p. 238). "All later religions prove to be . . . reactions aiming at the same great event with which culture began and which ever since has not let mankind come to rest" (p. 239).

This mere extrication and presentation of the framework of the Freudian hypothesis on the origin of socio-religious civilization is probably sufficient to prevent its acceptance; but a formal examination is only just.

First, the Darwin-Atkinson supposition is of course only hypo-



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thetical. It is a mere guess that the earliest organization of man resembled that of the gorilla rather than that of trooping monkeys.

Second, Robertson Smith's allegation that blood sacrifice is central in ancient cult holds chiefly or only for the Mediterranoid cultures of a certain period—say the last two thousand years B.C.—and cultures then or subsequently influenced by them. It does not apply to regions outside the sphere of affection by these cultures.

Third, it is at best problematical whether blood sacrifice goes back to a totemic observance. It is not established that totemism is an original possession of Semitic culture.

Fourth, coming to the Freudian theory proper, it is only conjecture that the sons would kill, let alone devour, the father.

Fifth, the fact that a child sometimes displaces its father-hatred upon an animal—we are not told in what percentage of cases—is no proof that the sons did so.

Sixth, if they "displaced," would they retain enough of the original hate impulse to slay the father; and if so, would the slaying not resolve and evaporate the displacements? Psychoanalysts may affirm both questions; others will require more examination before they accept the affirmation.

Seventh, granting the sons' remorse and resolve no longer to kill the father-displacement-totem, it seems exceedingly dubious whether this resolve could be powerful and enduring enough to suppress permanently the gratification of the sexual impulses which was now possible. Again there may be psychoanalytic evidence sufficient to allay the doubt; but it will take a deal of evidence to convince "unanalytic" psychologists, ethnologists, and laymen.

Eighth, if the band of brothers allowed strangers—perhaps expelled by their jealous fathers— to have access to the women whom they had renounced, and matrilinear or matriarchal institutions thus came into existence, what would be left for the brothers (unless they were able to be content with life-long celibacy or homosexuality), other than individual attachments to other clans; which would mean the disintegration of the very solidarity that they are pictured as so anxious to preserve, even by denying their physiological instincts?

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Ninth, it is far from established that exogamy and totem abstinence are the two fundamental prohibitions of totemism. Freud refers (p. 180) to Goldenweiser's study of the subject, which is certainly both analytical and conducted from a psychological point of view even though not psychoanalytical; but he fails to either accept or refute this author's carefully substantiated finding that these two features cannot be designated as primary in the totemic complex.

Tenth, that these two totemic taboos are the oldest of all taboos is pure assertion. If all other taboos are derived from them by displacement or distortion, some presentation of the nature and operation and sequence of these displacements is in order. An astronomer who casually said that he believed Sirius to be the center of the stellar universe and then proceeded to weave this opinion into the fabric of a still broader hypothesis, would get little hearing from other astronomers.

A final criticism—that the persistence into modern society and religion of this first "great event with which culture began" is an unexplained process—will not be pressed here, because Freud has anticipated it with a *tu quoque* (pp. 259–261): social psychologists assume a "continuity in the psychic life of succeeding generations" without in general concerning themselves much with the manner in which this continuity is established.

No doubt still other challenges of fact or interpretation will occur to every careful reader of the book. The above enumeration has been compiled only far enough to prove the essential method of the work; which is to evade the painful process of arriving at a large certainty by the positive determination of smaller certainties and their unwavering addition irrespective of whether each augments or diminishes the sum total of conclusion arrived at. For this method the author substitutes a plan of multiplying into one another, as it were, fractional certainties—that is, more or less remote possibilities—without recognition that the multiplicity of factors must successively decrease the probability of their product. It is the old expedient of pyramiding hypotheses; which, if theories had to be paid for like stocks or gaming cards, would be less fre-

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quently indulged in. Lest this criticism be construed as unnecessarily harsh upon a gallant and stimulating adventurer into ethnology, let it be added that it applies with equal stricture upon the majority of ethnologists from whom Freud has drawn on account of the renown or interest of their books: Reinach, Wundt, Spencer and Gillen, Lang, Robertson Smith, Durkheim and his school, Keane, Spencer, Avebury; and his special *vademecum* Frazer.

There is another criticism that can be leveled against the plan of Freud's book: that of insidiousness, though evidently only as the result of the gradual growth of his thesis during its writing. The first chapter or essay, on the Savage's Dread of Incest, merely makes a case for the applicability of psychoanalysis to certain special social phenomena, such as the mother-in-law taboo. In the second, the psychoanalytic doctrine of the ambivalence of emotions is very neatly and it seems justly brought to bear on the dual nature of taboo as at once holy and defiling. Concurrently a foundation is laid, though not revealed, for the push to the ultimate thesis. The third chapter on Animism, Magic, and the Omnipotence of Thought refrains from directly advancing the argument, but strengthens its future hold on the reader by emphasizing the parallelism between the thought systems of savages and neurotics. The last chapter is not, in the main, a discussion of the Infantile Recurrence of Totemism, as it is designated, but an analysis of current ethnological theories as to the origin of totemism in society and the presentation of the theory of the author. This hypothesis, toward which everything has been tending, does not however begin to be divulged until page 233; after which, except for tentative claims to a wide extensibility of the principle arrived at and some distinctly fair admissions of weakness, the book promptly closes without any reëxamination or testing of its proposition. The explanation of taboo on pages 52–58 is an essential part of the theory developed on pages 233 *seq.*, without any indication being given that it is so. Then, when the parallelism of savage and neurotic thought has been driven home by material largely irrelevant to the final and quite specific thesis, this is suddenly sprung. Freud cannot be charged with more than a propagandist's zeal and perhaps

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haste of composition; but the consequence is that this book is keen without orderliness, intricately rather than closely reasoned, and endowed with an unsubstantiated convincingness. The critical reader will ascertain these qualities; but the book will fall into the hands of many who are lacking either in care or independence of judgment and who, under the influence of a great name and in the presence of a bewilderingly fertile imagination, will be carried into an illusory belief. Again there is palliation—but nothing more—in the fact that the literature of theoretical anthropology consists largely of bad precedent.

But, with all the essential failure of its finally avowed purpose, the book is an important and valuable contribution. However much cultural anthropology may come to lean more on the historical instead of the psychological method, it can never ultimately free itself, nor should it wish to, from the psychology that underlies it. To this psychology the psychoanalytic movement initiated by Freud has made an indubitably significant contribution, which every ethnologist must sooner or later take into consideration. For instance, the correspondences between taboo customs and "compulsion neuroses" as developed on pages 43–48 are unquestionable, as also the parallelism between the two aspects of taboo and the ambivalence of emotions under an accepted prohibition (p. 112). Again the strange combination of mourning for the dead with the fear of them and taboos against them is certainly illumined if not explained by this theory of ambivalence (pp. 87–107).

It is even possible to extend Freud's point of view. Where the taboo on the name of the dead is in force we find not only the fear that utterance will recall the soul to the hurt of the living, but also actual shock at the utterance as a slight or manifestation of hostility to the dead. It is a fair question whether this shock may not be construed as a reaction from the unconscious hate carried toward the dead during their life, as if speaking of them were an admission of satisfaction at their going. The shock is certainly greatest where affection was deepest; persons who were indifferent are mentioned without emotional reluctance if circumstances permit, whereas enemies, that is individuals toward whom hate was

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avowed instead of repressed, may have the utterance of their names gloated over.

Of very broad interest is the problem raised by Freud's conjecture that the psychic impulses of primitive people possessed more ambivalence than our own except in the case of neurotics; that their mental life, like that of neurotics, is more sexualized and contains fewer social components than ours (pp. 111, 121, 148). Neurosis would therefore usually represent an atavistic constitution. Whatever its complete significance, there exists no doubt a remarkable similarity between the phenomena of magic, taboo, animism and primitive religion in general, and neurotic manifestations. In both a creation that has only psychic validity is given greater or less preference over reality. As Freud says, the two are of course not the same, and the ultimate difference lies in the fact that neuroses are asocial creations due to a flight from dissatisfying reality (p. 123). This is certainly not to be denied on any ethnological grounds; yet the implication that savages are essentially more neurotic than civilized men may well be challenged, although it cannot be dismissed offhand.

The experience of first-hand observers will probably be unanimous that primitive communities, like peasant populations, contain very few individuals that can be put into a class with the numerous neurotics of our civilization. The reason seems to be that primitive societies have institutionalized such impulses as with us lead to neuroses. The individual of neurotic tendency finds an approved and therefore harmless outlet in taboo, magic, myth, and the like, whereas the non-neurotic, who at heart remains attached to reality, accepts these activities as forms which do not seriously disturb him. In accord with this interpretation is the fact that neurotics appear to become numerous and characteristic in populations among whom religion has become decadent and "enlightenment" active, as in the Hellenistic, Roman Imperial, and recent eras; whereas in the Middle Ages, when "superstition" and taboo were firmly established, there were social aberrations indeed, like the flagellants and children's crusade, but few neurotics. Much the same with homosexuality, which the North American and Siberian natives have socialized. Its acceptance as

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an institution may be a departure from normality, but has certainly saved countless individuals from the heavy strain which definite homosexualists undergo in our civilization. It would be unfitting to go into these matters further here: they are mentioned as an illustration of the importance of the problems which Freud raises. However precipitate his entry into anthropology and however flimsy some of his syntheses, he brings to bear keen insight, a fecund imagination, and above all a point of view which henceforth can never be ignored without stultification.

While the book thus is one that no ethnologist can afford to neglect, one remark may be extended to psychologists of the unconscious who propose to follow in Freud's footsteps: there really is a great deal of ethnology not at all represented by the authors whom Freud discusses. To students of this side of the science the line of work initiated by Tylor and developed and most notably represented among the living by Frazer, is not so much ethnology as an attempt to psychologize with ethnological data. The cause of Freud's leaning so heavily on Frazer is clear. The latter knows nothing of psychoanalysis and with all acumen his efforts are prevailingly a dilettantish playing; but in the last analysis they are psychology, and as history only a pleasing fabrication. If psychoanalysts wish to establish serious contacts with historical ethnology, they must first learn to know that such an ethnology exists. It is easy enough to say, as Freud does on page 179, that the nature of totemism and exogamy could be most readily grasped if we could get into closer touch with their origins, but that as we cannot we must depend on hypotheses. Such a remark rings a bit naive to students who have long since made up their minds that ethnology, like every other branch of science, is work and not a game in which lucky guesses score; and who therefore hold that since we know nothing directly about the origin of totemism or other social phenomena but have information on these phenomena as they exist at present, our business is first to understand as thoroughly as possible the nature of these existing phenomena; in the hope that such understanding may gradually lead to a partial reconstruction of origins—without undue guessing.

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# NOTES ON ISLETA, SANTA ANA, AND ACOMA

By ELSIE CLEWS PARSONS

THE following data were got during a stay at Laguna. On Isleta, my informant was a Laguna man whom I shall call Felipe whose family had moved to Isleta and who had grown up and married there. The all too meager information about Santa Ana was from a Santa Ana man married to a Laguna woman. On my second visit to him he told me that his wife did not want him to talk—she is the daughter of one of the leaders in the ceremonial life of Laguna—and besides "some smart boys," as he called them, had advised him against talking to me, and, he might have added, frightened him. The refusal was characteristic. Pueblo Indians are quite as much afraid of being talked about as New England villagers or the smart set of a metropolis, and the charge of giving or selling information to a white is grave.—Information about Acoma clans is in part from notes made two years ago during a stay in Acoma. Other data about Acoma were got casually from some visiting Acoma women come to Laguna to trade, and more deliberately from an old Acoma acquaintance living at Acomita. As I had expected, he was more communicative away from Acoma than he had been in Acoma. Besides, he learned that the next day I was leaving the country. Even so, he balked about telling me even the group names of the *cheani* or medicine-men. Information is indeed so difficult to get from Acoma and certain pueblos that when any is obtained, fragmentary though it be, it should be presented, both for what value it may have in itself and because the only way to learn something from the Pueblo Indian, as from the secretive elsewhere, is to know something.

The clans (*daainĭn*) of Isleta are Day (*tŭ*), Bear, Lizard, Eagle, Chaparral-Cock, Parrot, Goose, Corn.[1] The first four are accounted

[1] A. F. Bandelier mentions fourteen clans. From the above list he omits Lizard, Chaparral-cock, and Parrot; he gives Sun instead of Day (see below), and he includes,



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Summer People, *shuren*, the last four, Winter People, *shifunin*. The clans are matrilineal and exogamous. The moieties are neither exogamous nor endogamous. The moieties, as far as I could learn, are divisions merely for ceremonial purposes.[1] Each has its own estufa or *tu ła* (Keresan *k'a'ach*), and its own headman, *i.e.*, *shurekabĕde* for the Summer People and *shifukabĕde* for the Winter People.[2] Each moiety will invite the other to participate in the dance it has in charge, *i.e.*, the Summer People in charge of

besides four corn clans, Deer, Antelope, Water, Elk, Moon, Duck. The first three clans, Deer, Antelope, Water, are found at Laguna and may represent Laguna immigrants. (*Final Report*, Pt. I, *Papers, Archaeological Institute of America* p. 273, American Series, III, 1890.)

[1] Cp. J. P. Harrington, "The Ethnogeography of the Tewa Indians," *Twenty-ninth Annual Report, Bureau of American Ethnology*, (1916), p. 62.

[2] These divisions, *shure* and *ship-hung*, Bandelier identifies with the *koshare* and *cuirana*. (*Final Report*, Pt. I, pl. 315.)

It seems likely that the summer-winter moiety pattern, however it originated or was applied among the Pueblo Indians, is carried throughout the pueblos in connection with the two groups of "delight makers." In Laguna there are traces of a long-standing fued between the *kurena* and the *kashare*, and indications here as elsewhere, according to Bandelier (*Final Report*, Pt. I, p. 301), that the *kurena*-Winter party was the progressive, and the *kashare*-Summer party, the conservative or anti-American party. References at Laguna to both the *shikani-kurena cheani* (also called *hadjamuni kaiuk*, Broken Prayer-stick) and the Flint *cheani* as being the paramount chief or cacique may point to the two-fold cacique system, although, on the whole, it would appear that the senior Flint *cheani* became the cacique with the *shikani-kurena* as his assistant.

The yellow altar is said to belong to the *kurena*, the blue to the *kashare*, the red to the Flint *cheani*, and the white to the Fire *cheani*.

At Zuñi the moiety pattern is seen in the assignment of two winter months in myth and ceremonialism to the *łewekwe* society. The myth of the separation of the *łewekwe* is in part like that given by Bandelier (*Final Report*, Pt. I, pp. 303–4) to explain the Tewa separation of the Winter and Summer peoples. The moiety pattern may also be seen in the requirement that at the summer rain dances the *koyemshi* must be in attendance. At the winter dances either set of "delight makers," *koyemshi* or *ne'wekwe*, may come out, and the *ne'wekwe* are particularly conspicuous.—By the way, may not *koyemshi* be derived from the Jemez variant for *koshare* (*Final Report*, Pt. I, p. 315), *kuenshare*?

Again at Zuñi the moiety pattern is or was seen in the extinct *łahewe* ceremonial, in the war dances, and in the Saint's dance. In the Saint's dance the six estufas are grouped into two alternating and more or less competing sets. They compete, for example, in furnishing "soldiers" for the Saint. It may not be insignificant that the regular leader of one set is the son of the *kyakweamosi łashi* who is to be equated with the head of the Flint society of the Keresans, *i.e.*, the cacique or, perhaps, Summer cacique. The dancer set meets in the house of the *kyakweamosi łashi*.

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the summer dances will invite the Winter People to participate, and *vice versa*.[1]

Classification by moiety does not enter into the *tablita* dance (maskless, with headdress of painted boards) which is danced by all together including the Laguna people settled at Isleta.[2] The *tablita* dance is performed Christmas and New Year's eves inside the church, as is the corresponding dance, the *talawaiye*, at Laguna, and the four succeeding days in the plaza.

At Isleta the image of the *santu* or saint is carried out on a circuit of about five miles through the country on the days of Little San Augustin, June 10, and of Big San Augustin, August 28 (?). The women carry the image as they do at Zuñi when *santu* is taken out to the fields in time of drought. The Isleta trip is made in the morning and in the afternoon, after food offerings for the *santu* are taken to the house of the "war captain" (see below), *tarawai* (*talawaiye*) is danced under the direction of the secular officers (see below) in the plaza where a bower has been made for the *santu*,

According to my informant, there are no masked dances at Isleta, there are no *k'atsina*. The only masks worn are those of the *teen*, equated with the *chapió* of Laguna[3] and Acoma, bugaboos the children are told are Mexicans. At Isleta the two *teen* are said to live in Matsena mountain (?); they wear white masks, they do not talk, and they serve as guards against intruders. Because there are no *k'atsina* at Isleta the Indians of the northern pueblos, I was told, call the Isleta Indians Mexicans, and will not admit them as onlookers at their own masked dances. In the Isleta dances the face is whitened and a feather headdress is worn.

[1] Cp. Noël Dumarest, "Notes on Cochiti," *Memoirs, American Anthropological Association*, vol. VI, no. 3, p. 184 n. 6.

[2] Classification by moiety occurs at Laguna in connection with the dances in which the alternating dance group figures in church feast dances. Among the East side people (*hanityumĕ*) are grouped the following clans: Sun, Corn, Eagle, Turkey, Water, Turquoise. West side people (*purnityumĕ*) are: Lizard, Bear, Parrot, Coyote, Chaparral Cock, Oak.

[3] The Laguna *chapió* wears long hair and a black faced mask with a white cross on the forehead. A white neck kerchief holds the mask in place. He carries a whip and ropes to bind the children. From his trousers he shakes out peaches and nuts for the children. It seems likely that *chapió* like *atoshle* of Zuñi is derived from *El Aguelo* (*El Abuelo*) of the Mexicans. A. M. Espinosa, ("New-Mexican Spanish Folk-Lore." *JAFL* 29 (1916), pp. 517–18). Cp., "Notes on Cochiti," pl. VII, fig. 2.

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With no *k'atsina*, there is of course no *k'atsina* organization; but the rest of the ceremonial organization, with perhaps one highly interesting exception, appears to correspond to the Keresan type of organization. There are two groups of *cheani* or medicine-men (*kaan*)—Flint (*dŭai kaan*), in Keresan *hishchean*, and Fire (*biŭre kaan*), in Keresan *hakani*; and there are, as among the Keresans, three "war captains," annually elective officers with ceremonial functions. In summer, in a drought, the war captains will ask the *kaan* to hold a ceremony. Then for four days the people at large may make no smoke out doors.[1] On the third day one group of men will clean up the town and another group will go on a hunt, the spoils to go to the *kaan*.[2] On the fourth night in the estufa the ceremony is held all night until dawn. The ceremony is open to all (excepting of course whites), even to the Laguna people of Isleta. In that great split of the medicine-men of Laguna a half century ago which led to migration to Isleta the Flint and Fire together with some of the *shahaiye* medicine-men were among the emigrants.[3] The Flint and Fire groups from Laguna consolidated with the same groups in Isleta. There is still one *shguyu* (giant) *cheani* (perhaps the *shahaiye* referred to, as the *shguyu* were a division of the *shahaiye*) in Isleta. He goes with the Fire society.[4]

The *kaan* use the two estufas already referred to—the Flint

[1] Cp. E. C. Parsons, "Notes on Zuñi," Pt. I, *Memoirs American Anthropological Association*, vol. IV, 1917, no. 3, p. 164.

[2] At Laguna the morning after the communal hunt (*oshach kauutsina goanya*, hunt in honor (?) of Sun) in connection with the summer solstice ceremonial, *i.e.*, the morning before the night of the all-night ceremony I saw the war captain go from house to house collecting the rabbits. The rabbits had been skinned. It is said that they are placed on the altar. The war captain on coming to the door will say to the woman of the house:

chauutawe hachtse tyieti' pedra tsuna
kill men rabbits jack rabbits prairie dogs

Formerly there were at Laguna, as there still are at Zuñi, *k'atsina* hunts. The war captain would take a package of tobacco to *k'atsina hocheni* (chief) and invite the *k'atsina* to go on a hunt.

[3] E. C. Parsons, "Notes on Acoma and Laguna," *American Anthropologist*, N.S., vol. XX, (1918), p. 184; "Ceremonialism at Laguna," *Anthropological Papers, American Museum of Natural History*, vol. XIX, pt. 3, pp. 108–9.

[4] My informant Felipe had been a Fire *cheani* at Isleta. He himself never mentioned the fact.

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society using that of the Day-Bear-Lizard-Eagle moiety, the Fire society that of the Geese-Corn-Chaparral-cock-Parrot-moiety. From this grouping as well as from the moiety clan classification, it occurs to me that my informant may have been twisted in his application of the summer and winter terms. It were consistent with Pueblo Indian classification in general for the Winter People to consist of the Day-Bear-Lizard-Eagle moiety and to be identified with the Flint Society, and for the Summer People to consist of the Geese-Corn-Chaparral-cock-Parrot moiety and to be identified with the Fire Society.

The altars (*geidĕ*) of the *kaan* seem to be quite like those of Laguna or Zuñi. They are ground altars of designs in meal and sand and of painted boards. With the meal and sand, black, white and yellow, cloud designs are drawn. The wooden frame is about ten feet long and four feet high. Faces and lightning symbols are painted on the boards, but not, as at Zuñi, the animals of the directions, nor are there carved figures of birds. In a space of about two feet behind the altar sit the *kaan*. On the altars stand *iamaparu'*, those most sacred fetichistic cotton-wrapped ears of corn the Keresans call *iyatik'* or *iariko* and the Zuñi, *mi'we*, together with stone figures of lion and bear and the uncarved stones, the Keresans call *samahiye* and the Isleta people, *wadaiyni*. The *wadaiyni* are dressed with feathers and beads, as at Laguna, but instead of the feathers of the *s'giti*, a large hawk, the feathers used are sparrow-hawk.—The fetich stone animal figure carried by hunters is of the wolf (*karnin*).

Besides the special ceremony for rain, the *kaan* regularly officiate at a winter solstice ceremonial and at a summer solstice ceremonial (*ibeweyuwe*, in Keresan *kuashi'wannatia*, "they act as rain clouds"). As part of the solstice ceremonials the *kaan* make prayer-sticks (*towai*) which are called *mapŭtowai* (*mapŭ*, ear of corn). Each *mapŭtowai* is accompanied by a crook stick similar to what at Laguna is called *hadjamuni kaiuk* (prayer-stick broken) and at Zuñi, the prayer-stick of the *pekwin* or sun priest.[1] These

[1] Details of feathers and pigments I reserve for a general account of prayer-sticks.

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*towai* are planted in the cornfields by the *kaan*. They are completely buried. They must incline or point towards the town.

Laymen do not make prayer-sticks. But feathered strings, *nashie'* (*wapanyi* in Keresan), are made by the clan heads. The clan heads make *nashie'* for a deceased clansman or woman, and on the fourth day after death they take out the *nashie'* together with food and deposit all in the river bank or under a fruit tree, a peach or apple tree.

The clan heads go into a retreat of continence and fasting for rain after the summer solstice. The Day clan is the first to withdraw. According to Felipe, the same system of ceremonial clan heads (four of them, all men, selected on a vacancy through death by the assembled clan, men and women, on the eve of solstice ceremonials) used to prevail at Laguna.[1] Beginning with the heads of the Sun clan (equated with the Day clan of Isleta—"the only difference is that we (of Laguna) say it out," *i.e.*, directly) all the clan heads (*hano nawaai*) went successively into a rain retreat of four days when they made prayer-sticks with the assistance of any clan members who volunteered. Some system of clan headship there undoubtedly was and in a measure still is in Laguna; but the account of this highly ceremonialized system, so strikingly like the Zuñi *ashiwanni* system, I was unable to verify from other informants and as the evidence goes I must hold that Felipe was reading the Tanoan system into Keresan custom.

As at Zuñi and at Sia, Cochiti, and Laguna there is at Isleta a ceremonial (*daikwan*)—here in March or April—for synchronous cures, an exorcising ceremonial to cure or clean the ground[2] of witch-sent worms or grasshoppers, and all sick persons of witch-caused disease (*lŭapŭ*, clean, person). For four days, beginning at night,[3] the *kaan* go into retreat. On the fourth day, about nine

[1] They had no altars and no *iyatik'* proper; but they kept in a basket the completely kernelled ears of corn (*kotona*) of which the *iyatik'* is made. The clan heads would send out to notify clansmen to bring to them all the *kotona* found in their harvest. The clan heads kept fetich animals (*shohuna*), also terraced medicine-bowls (*waitichaini*). Clan heads assisted the *cheani* at the winter solstice ceremonial to cut feather-sticks for the Sun and for property.

[2] Zuñi, *awek shuwaha*, ground, clean.

[3] Cp. "Notes on Zuñi," Pt. I, p. 52; M. C. Stevenson, "The Sia," p. 74.

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in the morning, from each set of *kaan* they go out, two by two, to each of the four directions, and with each set goes a "war captain." The *kaan* carry with them corn pollen which they sprinkle in the fields and the prayer-sticks known at Laguna as *chasumi* and used there by the war captains, a befeathered reed to which are attached netted shield, and miniature bow, arrow and club—the war god offerings. These things are buried in the fields. About four in the afternoon the *kaan* return, and that night they cure the people.

To each estufa[1] there are two rooms, an outer room and an inner. The people remain in the outer room together with the food which they have prepared according to their moiety, while in the inner room the *kaan* sing four or six songs. Then the war captain brings into the inner room those who are to be treated. The patients sit together, the *kaan* in the middle of the room. On this occasion the *kaan* will visit, one group the other, six *kaan* going from the Flint Society to visit the Fire Society and *vice versa*. In curing the *kaan* will look into a bowl of water on the surface of which "powder" is spread, in order to see into the machinations of the witches, senders, as always, of disease. The disease-causing things the witches have sent into the body the *kaan* take out with their eagle wing feathers—a familiar Pueblo Indian curing rite—and from the feathers are seen to drop into the *ollas* pebbles, bits of cloth, etc.,[2] and cactus points.[3] This curing or exorcising motion (*luati*; Keresan, *kukats*)[4] is a motion of sweeping in, "like catching a fly," and then shaking down, shaking the things caught from the feathers to the *olla*.[5]

Besides the clan heads and the *kaan* there is at Isleta the warrior organization which existed among the Keresans as *u'pi'* and still

*Eleventh Annual Report, Bureau of American Ethnology;* "Notes on Cochiti," pp. 204–5. At Laguna, at the all-night solstice ceremonials every one present is given a drink of medicine.

[1] The *tula* are referred to on this occasion, I think, but the *kaan* have their own ceremonial rooms.

[2] An identical rite of the Ant or Flint society of Zuñi.

[3] My informant insisted that these objects had been sent not into the body but into the clothes of the victim, next the skin, and "like germs" caused disease. Truly the Pueblo Indian is unsurpassable as a pourer of new wine into old bottles!

[4] *Tsyukats*, he has been cleaned; *tsaaukashana*, cleansing or "treatment."

[5] Cp. "Notes on Cochiti," p. 156.

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exists at Zuñi in the so-called bow-priesthood or *apilashiwanni*. Of these lifelong representatives of the war gods there are at Isleta about eight or nine. Nowadays these *kŭmpawi'lawen* are recruited through sickness. A sick man will think of becoming a *kumpawi'lawen*, if he recover, and he will make known his plan or, so to speak, vow, to his family. Relatives are summoned to talk it over and give consent. The invalid's father will go to the house of the *kumpawi'lawen* and present his son. The head of the *kumpawi'lawen* waits one day, and then calls together the members to tell them "he has received a new child."[1] Thereafter they will all pray for the invalid in their heart, wherever they may be, that is there is no ceremonial. If the invalid recovers, they hold an initiating dance. If he dies, they will be in attendance at the house and at the grave; so that people will know that the deceased belongs to the *kŭmpawi'lawen*.

The *kŭmpawi'lawen* assist at the *daikwan* ceremonial. Their own special ceremonial they hold in April in connection with the footraces. On the first and second Sundays the races are secular, on the third Sunday they are for the *kŭmpawi'lawen*. Until about four in the afternoon the *kŭmpwai'lawen* and volunteers dance, *tŭavără* the dance is called. The *kŭmpawi'lawen* are dressed in buckskin, carrying bows and arrows, with bandoliers crossed over their shirts, making the familiar war god design. There are two lines of male dancers, and women take part. After the afternoon race, the dance is renewed to continue all night.

The race is of the relay, queue or *chongo* catching type, made familiar by Lummis' lively description.[2] The winner's father takes a package of native grown tobacco to the estufa. The head *kŭmpawi'lawen* comes up and receives it and prays. Other *kŭmpawi'lawen* stand on the ladder to receive and pass down the presents of food and goods brought by the kinswomen of the winner.

[1] The phrase is current at Zuñi. Besides, when a society member happens to find anyone unconscious and restores him he may say, "I have found a child" and have him initiated into his society.

At Laguna the warrior who made a *coup* on a Navajo went into retreat for twelve days and chose a "brother" from the *u'pi'* with whom to exchange presents.

[2] *The Land of Poco Tiempo*, Chap. v. New York, 1897.

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Two *kŭmpawi'lawen* stand by the presents and after praying for a half hour or so take a bit of each kind of food from each basket, wrap the bits in wafer bread, and leave the roll at the foot of the ladder for the supernaturals. The defeated runner has to be "paid" with the presents made by the victor's circle of relatives and friends. Excepted are five baskets, of which one goes to the head *kŭmpawi'lawen*, one to his second, one to the war captains, one to the Flint Society and one to the Fire Society.

None of the races is run by moiety or by clan. On the fourth Sunday may be run a race between the married and unmarried men.

The secular officers of Isleta are *tabude* (governor; *tapup'*, Keresan and Zuñi), two *tinyientin* (lieutenant governors) and two *kaveun* (fiscals).

Santa Ana clans are: Dove (*hooaka*) or Snake which is the most numerous clan, Mouse, Coyote, Lizard, Bear, and Turkey which is the second clan in numbers, White Shell or Turquoise (Keresan, *yashje*, Mexican, *chucheita*)[1] Eagle, Corn, Water, Turquoise, Parrot, Fire (only two survivors), Ant. Sun, and Oak are extinct clans. As far as my informant knows, there never were Antelope or Chaparral Cock clans, clans found at Laguna, or a Tansy Mustard (*ise*) clan, a clan of Acoma, Zuñi, and of the Hopi. There is one Badger clansman, his mother from Zuñi.[2]. In the *santu* dance where, as usual, the pattern of alternating groups is followed, clan moieties appear. In one moiety are Dove, Mouse, Coyote, Lizard, Bear; in the other, Turkey, White Shell, Eagle, Corn, Water, Turquoise, Parrot, Fire. The Ant clan groups with either moiety.[3] Whether or not the moieties figure in other connections I did not learn. There are no clan heads or *hano nawai*, according to my

[1] The ambiguity here comes from the fact that the terms given by my informant for this clan refer to different things—*hasje'* means white-pink shell, and *chucheita* means turquoise. *Chucheita* or, as Bandelier gives it, *chalchihuite*, is, according to Bandelier, a Nahuatl word. (*Final Report*, Pt. 1, pp. 262–3.)

[2] All the Badgers of Laguna I know or have heard of are of Zuñi descent.

[3] In accordance with the Zuñi notion that ants are to be associated with all the directions. F. H. Cushing, "Zuñi Fetiches," *Second Annual Report, Bureau of American Ethnology* (1880–1), p. 16; M. C. Stevenson, "The Zuñi Indians," *Twenty-third Annual Report, Bureau of American Ethnology*, p. 409.

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informant; but understanding of the meaning of this term is hard to come at, so that unless the enquiry is followed up in particulars of personnel and of function, categorical statements remain uncertain.

The *cheani* groups are: Flint, Fire, Eagle, *shiwanna*, and two sets of *shika*.[1] There are no *saiyap* (*shumaekoli*) or *shahaiye* Giant *cheani*. The Flint *cheani* have to manage the *kŭshailĭ* (*kashare*)[2] and each set of *shika cheani* has *kurena*.[3] As at Laguna, to become a *shiwanna cheani* you must have experienced lightning-shock. My informant denied that the Eagle *cheani* were thought of, as at Jemez,[4] as eagle hunters or trappers. Their functions are the same as the functions of the other *cheani*—curing and weather control. As at Laguna[5] and Acoma, there are *sheék* or hunting

[1] Stevenson gives *shike*, for star, an extinct Star clan. ("The Sia," p. 19.) This may be the meaning of the *shika* cheani of Santa Ana and the *shikani* of Cochiti and Laguna. The *shikani-kurena* of Laguna called upon the stars in his solstice chant and had a right to the use of the cosmic symbols, sun, moon, and stars. ("Ceremonialism at Laguna," p. 108 n. 1.)

In this connection I may note that Stevenson for some obscure reason calls the *ne'wekwe* of Zuñi, the Galaxy fraternity. The *ne'wekwe* are the homologues of the Keresan *koshare* or *kurena*.

I also note that the *tzi-hui* medicine-man of the Tewa, corresponding, according to A. F. Bandelier, to the *shikani* (*shikama*) of the Keresans, has charge of the war god or star god fetiches, the name of one, *tzi-o-veno ojua* (Keresan, *shiwanna*) suggesting *chakwena*, the masked war group of *shiwanna* or *k'atsina* associated in ritual with the *shikani cheani* of Laguna. (*Final Report*, Pt. 1, pp. 305, 308–9.)

[2] In this connection compare the association of the Flint *cheani* and Summer people (the *koshare* being elsewhere identified with Summer people) at Isleta. There are suggestions of a sometime association between Flint *cheani* and *kashare* at Laguna.

[3] At Laguna and Cochiti the weather controlling, non-curing division of the *shikani* (*shikarne*) are *kurena* (*quirana*).

[4] "Notes on Cochiti," pp. 193–4. According to Felipe, eagle trapping by fastening a live rabbit in a pit is Najaho, and the Eagle *cheani* are derived from the Navajo.

[5] The *she'ek* (*shaiyaik*) knows the proper hunting songs, songs which make it easy to kill deer and rabbits, and how to make the hunter's feather-sticks. Four days before going on his hunt the hunter takes to the *she'ek* to set on his altar some micaceous hematite (*wakŭr*), representing the foot of the deer, red beads, (*yashjamutse wishtgŭrin*), representing the red flesh of the deer, flint (*hish*) representing the white on his neck and chest, and some turquoise (*shuimi*) or, if the quarry is an antelope, some white-pink shell (*yashja*) to represent the heart. Subsequently this shell mixture is taken on the hunt and deposited in the tracks of the quarry, at the frontal tip of the track. The feather-sticks are deposited in the lairs of the animals . . . . After the kill the head of the animal is pointed in the direction of the hunter's home. If the animal is still alive when caught, the muzzle is tied up close. (See Ceremonialism at Laguna p. 127.)



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*cheani*. The sequence of retreats for rain (*kuashiwanatyia*) after the summer solstice ceremonial is: Flint, *shiwanna*, Eagle, *shika*, *shika*.

The *kasik'*[1] or *tiamoni* does not have to be a *cheani*; but he is chosen by the *cheani*, by all the *cheani*. Formerly the *tiamoni* had an assistant or second;[2] nowadays he has none. He does not have to be chosen from any particular clan. As at Cochiti and Acoma and in modified form at Zuñi, the *tiamoni* selects the governor and all the annual officers—the *tapup*, one *tinienti*, two *piskales*, two *mayo roma* or ditch officers,[3] six *capitanes* to call meetings for the governor and to serve also for the ditch officers, and two *tsiyad-yuye'*, leaders (?), the first representing *maasewi*, the second or *tinienti*, *uyoyewi* together with their six officers who are called *tsamahiye*.[4]

Killers of Navajo are called *u'pi'*—as at Laguna—the killer of one Navajo, *u'pi' hocheni*, the killer of four Navajo, *naya*[5] *hocheni* (mother, head). Killers of the prey animals, lion, bear, wolf, and of eagles are also called *u'pi'*; but in some unexplained way their organization is thought of as a little different from that of the Navajo killers. On showing the skin of their trophy to the[6]

*She'ek* or *shaiyaik* and *shaikatsi* appear to be the names of the two heads of the group. Men became *she'ek* from being cured of sickness, any sickness. There is but one surviving *she'ek*; he lives at Parahi. His father was *she'ek* and in his father's day there five practicing *she'ek*. Then the *she'ek* held a dance in December—a dance *oshash kotsinia'* (Sun, for the sake of). The *she'ek k'a'ach* or meeting place was at the west end of *kakali*. The *she'ek* would call out the summons to the communal hunts saying: *Yunadish yunaponishaudyau yunakwichadyau yunahanichadyau hachtse shuichi dyanat-sashku dyiech pecha.* North, West, South, East, going to hunt, old man, young man, in four days, rabbits, jackrabbits. Before starting on the hunt the *she'ek* built a little fire so that the rabbits would not know where they (the rabbits) were going. A hunter might promise to himself to give meat from his hunt to the *she'ek*. The group is to be equated, no doubt, with the *tsaniakwe* of Zuñi.

[1] My informant asserted that the word was not "Mexican," but Indian.

[2] Cp. "The Sia," p. 16.

[3] Six ditch officers (*gwachani*, officer, *kopachawitse*, ditch) are elected at Laguna, elected in September, whereas the governor (*tapup*) his two lieutenants (*tyinyiintyi*), the two or, according to some, three fiscals (*fiskale*, *pishkali*, *piskare*) and the three "war captains" are elected on January 1.

[4] The same term, I believe, as that for the stone fetiches and we may infer that these are or were thought of as war god images.

[5] Cp. "Notes on Cochiti," p. 199.

[6] Cp. "Notes on Cochiti," p. 199.

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*tsiyadyuye'*, they would become *u'pi'*. As at Zuñi the skulls of the animals are deposited in a cave.

Including one *u'pi' k'a'ach* or *chiva* (Sp. *kiva*) there are five *k'a'ach*. They are associated with the *cheani—cheani k'a'ach*.

As well as I could make out the *k'atsina* or masked impersonations are likewise associated with the *cheani*. And impersonators were referred to as at Laguna as *k'atsina cheani*. The *k'atsina* function for rain, crops, and animals, and for the sick. The association of the *k'atsina* with the Antelope and Badger clans, a prominent association at Zuñi and Laguna, does not appear. These clans are not found at Santa Ana. The *cheani* and "*maasewi*" and "*uyoyewi*," are leaders for the *k'atsina*, and "*maasewi*" officers, the *tsamahiye*, their guards.

The clans of Acoma are: Antelope, Sun, Corn divided into Yellow Corn and Red Corn, Bear, Oak, Parrot, Chaparral Cock, Eagle, Turkey, Pumpkin (*tani*), Mustard (*ise*)[1] Snake,[2] Sky or Water. Extinct clans are Lizard[3] and Red Ant.[4]

[1] *Sophia halictorum* Cockerell, Mustard Family. Seed corn is "washed" or more probably sprinkled in a decoction of this plant to make it grow quickly. For a like practice with sage brush compare Stevenson, M. C. "Ethnobotany of the Zuñi Indians," *Thirtieth Annual Report, Bureau of American Ethnology*, (1915), pp. 87–8,

This clan is the *aiyahokwe* of Zuñi ("Ethnobotany of the Zuñi Indians," p. 86) and the *asa* of Hopi. *Aiyahokwe* and *asa* have been called Tansy-Mustard. An Acoma acquaintance supplied me with the specimen which was subsequently identified in the U. S. Dept. of Agriculture. Meanwhile I showed it to a Laguna acquaintance. *Shuma wawa* (dead, medicine) he called it. People drink a decoction of it to make them forget the dead. "Like *ise*, little different," he added. "I note that at Zuñi there is a mustard which is boiled and drunk by the *ne'wekwe* 'to loosen their tongues that they may talk like fools and drunken men.'" ("Ethnobotany of the Zuñi Indians," p. 91). This plant, I infer, is the *shuma wawa* of Laguna. I may say that *ise* was once described to me at Acoma as poisonous. I infer that the Laguna man mistook the piece of *ise* which I showed him for the other variety of mustard and I conclude that there are two varieties of mustard one of which the clan (*ise, aiyahokwe, asa*) is named after, and the other of which is used ritually in connection with the dead or by the foolhardy *ne'wekwe*.—We touch here upon an interesting although still obscure ceremonial complex. The *asa*, according to Fewkes a Tanoan clan that migrated to Laguna and thence to Zuñi and thence to Hopi, are identified with the *chakwena*. ("The Winter Solstice Ceremony at Walpi," *American Anthropologist*, vol. XI (1898), pp. 71–2). Now the *chakwena*, a war god cult at Laguna and Zuñi, are connected ceremonially with the *shikani-kurena* of Laguna who are to be equated with the *shiwannakwe* and *ne'wekwe* of Zuñi, organizations whose ritual may be seen to be associated with the dead and with war.

[2] Only one clansman left. [3] Identified by one informant with Snake.

[4] Bandelier mentions seventeen clans including four corn clans, and one name

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There is no division or alignment of the clans. In the *santu* dance and in the dance which I saw at Acoma,[1] the *hoinawe*, the two groups dance from the east and west estufas according to membership in the estufa,[2] and this membership is unrelated to clan membership.

The cacique or *kazik'* I met two years ago has since died. He was an Antelope clansman, and Johnson, my Acoma interpreter, was insistent that the cacique was always chosen from the Antelope clan and that this clan, *i.e.*, "the brothers and uncles" of the cacique had autocratic functions.[3] According to Johnson, his own father had been *kazik'* and he was succeeded by an uncle, his father's brother. Now the new cacique is Eagle, according to several Acoma women I met and according to Johnson himself when he began to tell me of the change in office. Later in the talk he said that the new *kazik'* (Francisco Wachampin) was Antelope (not a close relative of the deceased *kazik'*), it was his wife who was Eagle. A *kazik'* always takes his wife to live in the Antelope clan house appropriated to the *kazik'*. The contradiction is puzzling and, although I incline to think that Johnson was misstating in order to give prestige to the clan of which he was a child, more data are necessary. The history of the undoubtedly ceremonial position of the Antelope clan (and Badger clan) at Acoma as well as at Laguna and Zuñi is still obscure.

The *kazik'* watches the Sun. Unbroken continence is required of him. He does not have to be a *cheani*—in fact it is likely that he would not be one. He has no assistant or lieutenant. Together with the *tsatio hocheni* or "war captains" he looks after the *k'atsina* or masked impersonations.

not given. Clans not given in my list are Piñon-Eater, and Ivy (probably *ise* or Mustard. This plant was once called Ivy to me). Clans given in the above list and not by Bandelier are Oak, Sky, Mustard and, extinct, Red Ant and Lizard. (*Final Report*, Pt. I, p. 273. *Papers, Archaeological Institute of America*, Amer. Ser. III.)

Hodge mentions fourteen existing clans and six extinct clans. The extinct clans are Blue Corn, Brown Corn, White Corn, Fire, Buffalo and Ant. Of the clans in my list he omits Mustard and he does not identify Water with Sky. ("Pueblo Indian Clans," pl. VII, *American Anthropologist*, vol. IX (1896)).

[1] See "Notes on Acoma and Laguna," pp. 162–171.

[2] This is the Zuñi system when the pattern of alternating dance groups is followed.

[3] See "The Antelope Clan in Keresan Custom and Myth," *Man*, vol. XVII (1917), pp. 192–3.

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The *tsatio hocheni* is also, as might be expected, in charge of the communal hunts,[1] building the preliminary fire. There are also hunting officials, *shaiyaik*, who know hunting songs and how to make hunt prayer-sticks. My informant did not consider them to be *cheani*. Formerly there were the *u'pi'*, the scalp-taking warrior organization, but now "they are all gone."

About the *k'atsina*, my informant was almost as reticent as about the *cheani*. Women are never made *k'atsina*. The age of "making new *k'atsina*," *i.e.*, of initiation appears to be later than I once heard[2]—it is seventeen or eighteen or even later. As I mentioned the different sets of *k'atsina* figuring at Laguna—it was acknowledged that they figured also at Acoma—*gopeuts* (or *hematatsi*, Zuñi *kokokshi* or *upikaiupona*), *waiyush* (Duck, Zuñi *muluktakya*), *kaiya* (Mixed. Zuñi, *wotemła*), *hemish* (Zuñi; *hemushikwe*).[3] The existence of *gumeyoish* (Zuñi *koyemshi*, the masked clowns) and of *shonata*[4] who corresponds to *shulawitsi*[5] of Zuñi was also admitted. *Chapio'*, being a maskless Mexican[6] figure, was mentioned more freely. He rides a horse, a real horse, not a mock pony as at San Domingo.

NEW YORK CITY.

[1] "Notes on Acoma and Laguna," p. 173.

[2] "Notes on Acoma and Laguna," p. 174 n. 1.

[3] The *chakwena* I overlooked.

[4] The impersonator is always a Corn clansman. *Shonata* "belongs to the Corn clan." The same man habitually impersonates. When he dies or were he sick or absent another Corn clansman volunteers.

[5] Formerly at Laguna there was another mask, *shuraidja*, that seemed, in name at least, to correspond even more closely.

[6] At Laguna I have heard a mother threaten a boy under two with *chapio'*.

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# CHARLES C. ABBOTT

By CLARK WISSLER

DR. CHARLES C. ABBOTT, noted for his researches in the Delaware Valley, died at Bristol, Pennsylvania, July 27, 1919. He was born June 4, 1843, upon the site afterwards made famous by his discoveries. He lived in the old Abbott homestead where his ancestors settled in colonial days. This interesting old house contained many old and rare pieces of furniture, not to mention autograph letters and scientific mementos gathered by Dr. Abbott during his long and notable career. The house and its entire contents was destroyed by fire in 1914 and now stands as a deserted ruin. The loss was a great blow to Dr. Abbott, one from which he never fully recovered.

The degree of Doctor of Medicine was conferred upon Dr. Abbott at the University of Pennsylvania, 1865. From 1876 to 1889 he was assistant in the Peabody Museum at Harvard and from 1889–1893, curator of archaeology in the University Museum, Philadelphia. His first archaeological discoveries seem to have been finds of crude argillite implements on his homestead at Trenton. These were followed up by other discoveries, until in 1883, he developed the conception of three superimposed cultures in the soil of his estate. These observations stimulated the systematic investigation of the vicinity by Volk and others, on account of which the Abbott farm became one of the most famous in American archaeology.

The best known book of Dr. Abbott is his *Primitive Industry*, but he was the author of a number of archaeological publications, the most important of which are the following:

The Stone Age in New Jersey (American Naturalist, vol. VI, pp. 144–160, 199–229, 1872: also Annual Report for 1875, Smithsonian Institution, pp. 246–380, Washington, 1876).

Reports on the Discovery of Supposed Paleolithic Implements from



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the Glacial Drift, in the Valley of the Delaware River, near Trenton, New Jersey (Reports, Peabody Museum, vol. II, pp. 30–43, 225–257, Cambridge, 1880).

An Historical Sketch of the Discoveries of Palaeolithic Implements in the Valley of the Delaware River (Proceedings, Boston Society of Natural History, vol. XXI, pp. 124–132, Boston, January 19, 1881).

Primitive Industry. Salem, 1881.

Traces of a Pre-Indian People (Popular Science Monthly, vol. XXII, pp. 315–322, New York, 1883).

Evidences of the Antiquity of Man in Eastern North America (Proceedings, American Association for Advancement of Science, vol. XXXVII, pp. 293–315, Salem, 1888).

The Descendants of Palaeolithic Man in America (Popular Science Monthly, vol. XXXVI, pp. 145–153, New York, 1890).

Recent Archaeological Exploration in the Valley of the Delaware (Publications, University of Pennsylvania, Series in Philology, Literature, and Archaeology, vol. II, pp. 1–30, Boston, 1892).

Archaeologia Nova Caesarea. Trenton, 1907.

Ten Years' Diggings in Lenape Land, 1901–1911. Trenton, 1912.

AMERICAN MUSEUM OF NATURAL HISTORY,
NEW YORK CITY, N. Y.

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# BOOK REVIEWS

## METHODS AND PRINCIPLES

*Die ethnologische Wirtschaftsforschung: eine historisch-kritische Studie.* P. WILH. KOPPERS, S. V. D. (*Anthropos*, vol. X–XI, 1915–16, pp. 971–1079.)

Owing to the present capriciousness of trans-Atlantic transportation, the predecessors of the above-cited concluding instalment of a longer article remain inaccessible to the reviewer. Since, however, it constitutes an independent unit and embodies theoretical views of considerable importance, it seems permissible to furnish a brief indication of its contents.

Father Koppers, as his subtitle indicates, is concerned mainly with an historical summary of past opinions, together with a critical appreciation of past achievements. He has taken enormous trouble to represent with accuracy the views of many writers whose activities fall beyond the sphere of ethnology proper, such as geographers, historians, and economists; and it is impossible not to accord the highest praise to the manner in which he has acquitted himself of his arduous task. If any criticism is to be offered as to the form of his presentation, the reviewer would suggest somewhat greater compactness. Inasmuch as Father Koppers is not, after all, offering a psychology of scholarly investigation, much space might have been saved by a purely topical discussion instead of the inevitably lumbering method of considering each thinker individually.

The author's general point of view is that of Graebner, especially as later adopted by Father Schmidt. The latter, it appears, has developed a comprehensive scheme of cultural history, interrupted in publication by the war but largely drawn upon by Koppers. Indeed, the exposition of Schmidt's conceptions and the critique of Hahn (979 *seq.*, 1056 *seq.*) will prove of greatest interest to the readers of this journal.

As for Hahn's conceptions, Koppers accepts his critique of the time-honored three-stage theory, but repudiates the notion that pastoral life represents a secondary development from an agricultural mode of subsistence and the hypothesis that all domestication is based on that of the ox. Further the author rejects as superfluous and inherently improb-



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able Hahn's view that religious motives were instrumental in the primeval processes of domestication. Not that he postulates an intentional compulsory act: he rather reverts to occasional suggestions of Hahn himself relating to the corralling of game animals by hunting tribes (p. 992).

Schmidt's theory is expounded as follows (pp. 988, 1056). Graebner had assumed a bifurcation of cultural evolution from the primeval hunting state. As one branch he postulated a matrilineal moiety system correlated with the origin of horticulture through female effort; as another a patrilineal totemistic culture, from which he derived the civilization of pastoral peoples. It is this genetic connection between stock-breeding and totemism that Schmidt denies. In his scheme pastoral conditions represent a third independent post-primeval cultural province, that of northern Central Asia (southwestern Siberia). The marginal peoples whose culture represents that from which pastoralism evolved are the Eskimo, Lapps, Ainu, *et al.* Schmidt distinguishes three pastoral groups. The northeastern division embraces the bulk of Ural-Altaian stocks and the core of the cultural province; to these peoples must be credited the domestication of the reindeer, the horse, and the camel. The Indo-Europeans form the central group, their home being placed in northern Turkistan and southern Russia. As an as yet undifferentiated stock (*als einheitliches Gesamtvolk*) they adopted husbandry, though only in slight degree, from neolithic neighbors in Turkistan Finally Schmidt recognizes a southwestern division of Hamito-Semitic nomads, who only secondarily borrowed domesticated species from the two other groups.

A disturbing feature of Father Koppers' argumentation consists in the constant assumption of Graebner's cultural strata as definitely established historical facts. Yet it is quite possible to accept the notion of cultural stratification without recognizing in Graebner's totemistic and two-class cultures anything but speculative constructions. Apart from this important qualification, the reviewer gladly hails this paper not only as a learned and able essay on one of the most important aspects of material culture but also as embodying a number of eminently just methodological aperçus.

ROBERT H. LOWIE

POSTSCRIPT

Since writing the above I have had an opportunity of reading the first part of Father Koppers' study (*Anthropos*, vols. X–XI, 1915–16, pp. 611–651). It contains a number of interesting historical points. In opposition to Roscher, who ascribes the origin of the three-stage

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theory to Plato, Koppers finds that Plato recognizes but two epochs, that of the hunting herders being distinguished from tillers of the soil. The earliest representative of the theory seems to be Dicaearchus, a disciple of Aristotle who died about 320 B.C. In his scheme the first period is that of a paradisaical golden age, while some other writers of antiquity substitute an animal-like existence. Later speculators inclined definitely to the familiar classification into a hunting, a pastoral, and an agricultural stage, such as is found in Adam Smith's epoch-making work. This scheme was first challenged in 1786 by I. Iselin, apparently a Swiss writer, who noted the absence of cattle among the Maori as contrary to the assumed sequence. He thus preceded A. von Humboldt, who utilized primarily American data in rejecting the necessity of an intermediate pastoral stage.

In the four decades following the middle of the nineteenth century the author recognizes two antagonistic tendencies, the evolutionary and the historical. Koppers is severe, though not unjust, in his treatment of Morgan and his slavish followers; he explains the belated retention of the three-stage theory in France by the preoccupation of French scholars with archaeological rather than ethnological data. However, Koppers insists that Hahn exaggerates in assuming that the acceptance of the old scheme was everywhere general during this period. For one thing, the older historical school of political economists, notably Hildebrandt, Knies, and Roscher, entertained sound methodological principles hostile to a priori constructions of stages. Miss Buckland distinguished a lower and a higher form of husbandry and assumed a more or less historical position. Among other things she associated women with primitive tillage, a conclusion already clearly set forth in Bachofen's famous work. Finally Nowacki and Ling Roth are mentioned as anticipating some of the results of modern ethnology.

R. H. L.

*Americanization.* Carol Aronovici. Keller Publishing Co., St. Paul, 1918. Pp. 62.

Dr. Aronovici's book on Americanization is an attempt to give a scientific basis to the problem of racial amalgamation.

In the first place, he shows that a deliberate Americanization plan, in the sense of an attempt to force the ideas and ideals of ourselves upon other peoples, is not essentially different in spirit or aim from the Germanization schemes that we have vigorously denounced. The same nationality fever is largely responsible for both of them.

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Two points made by Dr. Aronovici are of particular interest to anthropologists: the assertion that in racial assimilation the higher classes of the respective races intermarry more readily than do the lower—owing, we may surmise, to the sharing of a common culture and a common language by the higher classes, whereas the lower have no such common basis of understanding. The other point is the assertion that racial differentiation in a given national group means mental stimulus.

As to the latter it seems to me that the author has not established his point. He gives as instances the racial differentiation of the British Isles, particularly that of England. But is this true of the majority of the great civilizations—of the Egyptians, the Hebrews, the Greeks, and the Romans? He himself refers to the cultural enrichment which those civilizations received from outside sources; but this can scarcely be taken as supporting his argument. Nor, for that matter, have we ever had in history such extremes of racial differences as have been thrown haphazard into the American melting pot.

After all, is it not a question of cultural diffusion? This is sometimes more easily effected from a distance than when the people are living side by side. The author makes the point, however, that when living side by side they have all the better opportunities for a culture borrowing and mutual enrichment. Therein lies the great opportunity of American civilization if it but recognize these opportunities and cultivate a receptive attitude that will utilize them. Some of this utilization of aboriginal culture we are now witnessing.

WILSON D. WALLIS

## NORTH AMERICA

*Handbook of Aboriginal American Antiquities. Part I, The Lithic Industries.* W. H. HOLMES. Bulletin 60, Bureau of American Ethnology, XVII + 380 pp. Washington, 1919.

This is the third in the series of Handbooks projected by the Bureau of American Ethnology. In order of their appearance, these are: *Handbook of American Indians North of Mexico* (Bulletin 30); *Handbook of American Indian Languages* (Bulletin 40), Part I of which has been published; and the work represented by the present volume. No living author is better informed on the subject of aboriginal American antiquities than William H. Holmes, who has the added advantage of being at once master of the philosophic and the systematic method of presentation.

In dealing with archaeology, the sources of information are com-

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prised under two heads: Intentional records and fortuitous records. Intentional records include: pictorial, monumental, quipu and wampum, oral, and written. Fortuitous records are the products in general of human handicraft, to which no mnemonic significance has ever been attached; the non-material results of human activity as embodied in beliefs, customs, music, philosophy, etc.; unpremeditated memories which accrue to each generation and are in part transmitted adventitiously, etc.

All that archaeology gathers from a wide field of research is contributed to the volume of written history. It is not only the "retriever of that which was treasured and lost, but equally the revealer of vast resources of history, of which no man had previously taken heed."

In the sense that what archaeology retrieves and reveals at once becomes a part of history, the author believes the terms "prehistory," "prehistoric period," and "prehistoric archeology" should not be employed without first fully setting forth their particular application. Concrete examples are cited to illustrate the relation of history and so-called prehistory and to show that archaeology is not limited to antiquity. In point of fact, the true prehistoric never ends, the archaeologist having before him an unlimited future and behind him an inexhaustible past.

Numerous and dangerous are the sources of misinformation, such for example are the "misinterpretations and errors embodied in four centuries of literature" pertaining to the New World. Reference is made to a number of popular archaeologic fallacies, which long resisted the progress of scientific research.

The Old World is accepted as the place of origin of mankind. It is inconceivable that the New World with its homogeneous physical type and imperfectly developed culture should have peopled the Old World with at least three racial types comprising the bulk of the world's population and progress.

Among the possible gateways from the Old World to the New, no other can compare with Bering Strait. The distance from land to land is only 40 miles and during part of the year even this is completely bridged by ice.

The work is written on the assumption that aboriginal American culture does not antedate the neolithic and that artifacts, which are palaeolithic in form are not chronologically separable. If palaeolithic man did not exist in the New World, then claims for Tertiary man in the auriferous gravels of California, South America, etc., must go by the board. The author admits, however, that "men have dwelt in the Dela-

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ware valley as elsewhere in America" for thousands of years, possibly as far back as the closing stages of the glacial period in the northern United States; but he does not believe the evidence thus far furnished as proof of the glacial or immediately postglacial occupancy to be conclusive.

A chapter is devoted to culture characterization areas. Keeping in view the archaeological rather than the ethnological evidence, twenty-two areas are recognized; eleven of these are north of Mexico: (1) The North Atlantic area; (2) the Georgia-Florida area; (3) the Middle and lower Mississippi Valley area; (4) the Upper Mississippi and Great Lakes area; (5) the Great Plains and Rocky Mountains area; (6) the Arid area; (7) the California area; (8) the Columbia-Fraser area; (9) the Northwest Coast area; (10) the Arctic Coast area; (11) the Great Northern-Interior area. For Middle and South America the areas are: (12) North Mexican; (13) Middle Mexican; (14) South Mexican; (15) Maya-Quiché; (16) Central American-Isthmian; (17) North Andean-Pacific; (18) Middle Andean-Pacific; (19) South Andean-Pacific; (20) Amazon Delta; (21) Primitive South America; (22) West Indian or Antillean. The areas are mapped; but the numbers XX and XXI should be transposed in figure 42 if the map is to coincide with the text; or else the subheading on page 141 should be number 21, and that on page 142 should be number 20; likewise the numbers 20 and 21 on page 97 should be transposed.

The first step in the consideration of antiquities is their classification. Of the available methods the author recommends classification by: (1) Geographic areas; (2) Culture areas; (3) Peoples; (4) Sequence; (5) Kind; (6) Materials; (7) Activities; (8) Culture steps; (9) Function or use.

The acquirement and utilization of materials is given exhaustive treatment, particularly mineral substances: "Stone in its various forms— useful, semiprecious, and precious; clay, salt, sulphur, alum, asphaltum, and pigments; gold, silver, copper, tin, quicksilver, meteoric iron, and iron ore, the latter treated and employed always as stone."

The more important quarry and mine sites are discussed at length: Quartzite Bowlder Quarries, District of Columbia; Flint Ridge and Warsaw Quarries, Ohio; Mill Creek Quarries, Illinois; Flint Quarries, Crescent, Missouri; Novaculite Quarries, Arkansas; Chert Quarries of the Great Plains; Quartzite Quarries, Wyoming; Obsidian Mines in the United States and Mexico; Red Pipestone Quarry, Minnesota; Hematite Ore and Paint Mine, Missouri; Steatite Quarries, Mica Mines; Turquoise Mines, Quarries of Building Stone.

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In the final chapters there is an illuminating account of the stone-shaping arts; the various processes of fracture, crumbling, abrading, incising, and piercing. The volume is made doubly attractive by numerous and well-chosen illustrations. The author is to be congratulated on having completed so auspiciously the first volume of the *Handbook of American Antiquities.*

GEORGE GRANT MACCURDY

*A Prehistoric Iroquoian Site.* A. C. PARKER. Researches and Transactions of the New York State Archaeological Association, Morgan Chapter, vol. I, no. 1. Rochester, New York. 1918.

*A Contact Period Seneca Site.* A. C. PARKER. *Ibid.*, vol. I, no. 2. Rochester, New York. 1919.

Ever since the veterans of Sullivan's army, who had stormed and burned the native villages in the Iroquois country in 1789, returned to take up their land grants in central and western New York, the attractive relics of the Iroquois have been a prey to the curio seeker. Site after site of incalculable importance to science has been ruined by the clumsy spades of untrained, unobservant diggers, so that today, one hundred thirty years later, the greater number of Iroquoian graveyards are completely looted, the bones of the dead lie scattered on the surface, and the specimens of native manufacture taken from them have been sold or given broadcast half-way around the world.

The task of gathering up the scanty crumbs of data from any given site, as the writer knows from his own field experience, is an onerous one, and it is particularly gratifying to receive from the pen of the man who best knows the Iroquois and their archaeology, the two pamphlets named above, fragmentary though their contents must needs be.

The first booklet concerns a well-known prehistoric Seneca fort on the Reed farm at Richmond Mills, Ontario county, New York, not far from the outlet of Hemlock lake. The site itself, as is usual with Iroquois sites of the period under discussion, occupies a high sandy knoll between two deep ravines, thus being easily fortified against incursions by the Algonkian tribes, whose remains dot the nearby fields. It covers an area of five acres, and, judging by its extensive ash-beds and sidehill refuse dumps, was long and intensively occupied. The cemetery lies across a ravine, but as usual no objects occur with the dead.

In the ash and refuse heaps, however, quantities of pottery sherds, bone and antler utensils; chipped, rough, and polished stone implements occur. Some five effigy pipes, as well as others in terra-cotta and in

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stone have been obtained. The usual Iroquois profusion of bone and antler artifacts is found, including fish-hooks, harpoons, both bilateral and unilateral, awls, beaming tools, needles, and combs, at least one of the latter being of the effigy type. Jinglers of deer phalanges, antler gaming disks, perforated teeth of the elk and bear, are among the objects listed.

The usual tiny triangular arrow points of flint have been noted, but the most significant remains are the sherds of the clay jars, which have the notched angle, peaked rim, constricted neck, and round bottom of the more eastern Iroquois forms. Rudely modeled human faces often appear at the collar projections. In size the vessels were larger than those commonly used by the later Seneca.

The little booklet draws to its close with a vivid "Visualization" of the ancient Indian life on the site, and an excellent summary of the facts.

The second paper deals with all available data on an early colonial Seneca site, now virtually destroyed by vandals, at Factory Hollow, Ontario county. The site was probably a flourishing Indian settlement when Denonville raided the Seneca country, but it escaped his attention, and was finally abandoned for some reason long ago forgotten.

The cemeteries of this site have yielded beautiful pipes, pottery vessels, bone combs, dolls, and many articles of commerce with the whites, such as glass beads and brass kettles. The pottery from the graves is smaller and more bowl-shaped than that found at Richmond Mills or indeed on the old ash-beds of the Factory Hollow site itself, which resembles the prehistoric ware. Mr. Parker says, as the reviewer believes, with justice, "during the occupancy of this site the Seneca Indians evolved or adopted another type of pottery decoration and even altered the form previously used." That this change in pottery forms was made at some period of their existence by the western Iroquois, all must admit who are familiar with the archaeology of the people. Mr. Parker, on the evidence furnished by this site, places it circa 1656.

Both the papers are enriched by generous illustrations and maps with further data by Messrs. J. C. Follett and W. H. Cassebeer.

It is the hope of all students of New York archaeology that the Morgan Chapter of the New York State Archaeological Association will continue to publish articles of this welcome nature, not only from the pen of America's foremost student of the Iroquois, but also from other members as well qualified to speak as Messrs. Follett and Cassebeer.

ALANSON SKINNER

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## OCEANIA

*Vorläufiger Bericht über Forschungen im Innern von Deutsch-Neu-Guinea in den Jahren 1913–1915.* R. THURNWALD. (*Zeitschrift für Ethnologie*, 1917, pp. 147–179.)

With his collections and notes scattered in half a dozen places Dr. Thurnwald labored under considerable difficulties in trying to present to his colleagues a preliminary report of his activities in New Guinea. Nevertheless his lecture contains a number of highly interesting descriptive data, to which it is desirable to call the attention of American students. It is a matter for congratulation that his researches on the social organization of the Bánaro are already accessible to us in English guise (*Memoirs of the American Anthropological Association*, vol. III, no. 4, October-December, 1916.)

Dr. Thurnwald's travels embraced both the lower and upper reaches of the Augusta river. Somatically he found a very large number of pygmies—even of individuals falling below 140 cm.—but these invariably occurred in the same communities with natives of medium stature. Another remarkable trait is the wide diffusion of albinism, which, to be sure, is of the "moderate" type. Neuhauss's characterization of the hair as blonde is rejected, since it is really only brown. Culturally Thurnwald distinguishes an Eastern and a Western zone, the former being far more highly developed along industrial and artistic lines; a steppe area is also recognized. Correlated with the cultural there is a linguistic difference, though throughout Papuan languages occur.

Economically the natives depend on yams, sago, bananas, breadfruit, sugar-cane, and in the mountainous districts on taro as well. Pigs and dogs are universally domesticated. The author devotes special attention to the house types. Pile-dwellings occur everywhere except in the steppes, where the roofs extend to the ground. However, the character of the pile-houses varies enormously, as illustrated by a number of photographs. The eastern house is on the whole characterized by an elevation of from one to two meters and rests on a few substantial forked posts. In the west the piles rise to the height of eight, twelve, and even fifteen meters but are very rarely forked, and accordingly the entire structure is lacking in stability. Here dwellings are communal and there is no distinct council house. About the sources of the Augusta river the posts do not exceed half a meter in height and the structure has a square groundplan, the whole resembling a cube with a prismatic gable roof.

Thurnwald examines the supposed correlation of pile-dwellings and the "bow-culture." His data from the western zone support the theory

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since bow and arrow constitute the dominant weapons, with bone daggers playing a secondary part. In the steppes the negative correlation demanded by the theory is also fairly well borne out, since the bow is almost completely supplanted by the spear except in ritualistic performances. But the eastern area provides contradictory evidence. Though pile-dwellings occur, bows are lacking in many districts, *e.g.*, wholly along the lower Augusta and the neighboring coastal tract, where darts are hurled from a throwing-board. This device is characteristic of the entire region, but the spear plays a significant part and is associated with clubs and bone daggers. Bows do not turn up until one reaches the middle course of the Augusta and of the Potters' river (Töpferfluss). Near Angroman the author found halberd-like wooden weapons, which suggested those from the islands of Aua and Wuvulu.

A remarkable form of shield made from the feathers of cockatoos, parrots, birds of paradise, and other species was discovered on the Potters' river and nearly a hundred specimens were collected for the Berlin Museum. The author considers them among the finest achievements of Oceanian art. They served purely ceremonial functions and were carefully stored in the council houses.

In conclusion Thurnwald insists on the quite distinct processes involved in the transmission not only of material and non-material aspects of culture but even in the diffusion of different elements of material culture. In this connection he also avows his belief in the possibility of independent invention.

ROBERT H. LOWIE

## SOME NEW PUBLICATIONS

Barrett, S. A. and Hawkes, E. W. The Kratz Creek Mound Group; a Study in Wisconsin Indian Mounds. (Bulletin of the Public Museum of the City of Milwaukee, vol. III, no. 1, pp. 1–138, pls. 1–19, figs. 1–19.) Milwaukee, 1919.

Cory, Herbert Ellsworth. The Intellectuals and the Wage Workers; a Study in Educational Psychoanalysis. New York: The Sunwise Turn, 1919. 273 pp.

De Booy, Theodoor. Archaeology of the Virgin Islands. (Indian Notes and Monographs, vol. I, no. 1, pp. 1–100.) Museum of the American Indian, Heye Foundation: New York, 1919. 10 pls., 32 figs.

——. Santo Domingo Kitchen-midden and Burial Mound (*ibid.*- vol. I, no. 2, pp. 101–137.) New York, 1919. 15 pls., 9 figs.

Fewkes, J. Walter. Prehistoric Villages, Castles, and Towers of



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Southwestern Colorado. (Bureau of American Ethnology, Bulletin 70.) Washington, 1919. 79 pp., 33 pls., 18 figs.

Hay, Oliver P. On Pleistocene Man at Trenton, New Jersey. (Anthropologic Scraps, no. 2, Dec. 3, 1919, 4 pp.)

Hodge, F. W. Bibliography of Fray Alonso de Benavides. (Indian Notes and Monographs, vol. III, no. 1, 39 pp., 5 pls.) New York: Museum of the American Indian, Heye Foundation, 1919.

Holmes, W. H. Handbook of Aboriginal American Antiquities. Part 1: Introductory; The Lithic Industries. (Bureau of American Ethnology, Bulletin 60.) Washington, 1919. 380 pp., 223 figs.

Kronfeld, E. M. Der Krieg im Aberglauben und Volksglauben. Kulturhistorische Beiträge. 1915.

Laufer, Berthold. Sino-Iranica. (Field Museum of Natural History, Publication 201, Anthropological Series: vol. XV, no. 3, pp. 185–630.) Chicago, 1919.

Leguizamon, Martiniano. Etnografia del Plata: El Origen de las Boleadoras y el Lazo. (De la Revista de la Universidad de Buenos Aires, vol. XLI, p. 206 *seq.*) Buenos Aires, 1919. 53 pp.

Lenhossek, M. v. Die Zahnkaries einst und jetzt. (Archiv für Anthropologie, vol. XVII, 1919, pp. 44–66.)

Lenz, Rodolfo. Sobre el Estudio de Idiomas. Carta al Señor don Julio Saavedra Molina. Santiago, 1919. 127 pp.

Lothrop, S. K. The discovery of Gold in the Graves of Chiriqui, Panama. (Indian Notes and Monographs, vol. VI, no. 2, pp. 25–34.) New York, 1919. 3 pls. 1 fig.

Maass, A. Quer durch Sumatra. Reiseerinnerungen. 2. Auflage. 1917. 41 ills.

Mason, J. Alden. Tepecano, a Piman Language of Western Mexico. (Annals of the New York Academy of Sciences, vol. XXV, pp. 309–416.) New York, 1917.

Michelson, Truman. Some General Notes on the Fox Indians. Part III, Bibliography. (Journal of the Washington Academy of Sciences, vol. IX, no. 19, pp. 593–596.)

Montandon, George. La Généalogie des instruments de musique et les Cycles de civilisation. (Archives suisses d'Anthropologie generale, vol. III, 1919, no. 1, pp. 1–118.)

Oetteking, Bruno. The Processus Frontosphenoidalis of the Zygoma and its Bearing on the Configuration of the Orbit. (Anatomical Record, vol. XVII, 1919, pp. 25–31.) 3 pls.

Pan, N. Some Observations on the Gastro-Intestinal Tract of the

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Hindus. (Journal of Anatomy, vol. LIII, pt. II and III, April, 1919, pp. 259–265.)

Richet, Ch. Allgemeine Kulturgeschichte. Versuch einer Geschichte der Menschheit. Deutsch von R. Bergen. 1918.

Saville, Marshall H. Archaeological Specimens from New England. (Indian Notes and Monographs, vol. V, no. 1, 1–10 pp. See De Booy, Theodoor.) New York, 1919.

——. Bibliographic Notes on Quirigua, Guatemala. (Indian Notes and Monographs, vol. VI, no. 1, 22 pp.) Museum of the American Indian, 1919.

Schulz, Gustav F. The Cripple in Primitive Society. (The American Journal of Care for Cripples, vol. VIII, 1919, pp. 335–346.)

Skinner, Alanson B. Some Menomini Indian Place Names in Wisconsin. (Wisconsin Archeaologist, vol. XVIII, 1919, pp. 97–102.)

——. Recent Mound Explorations in Shawano County. (*Ibid.*, pp. 105–107.)

——. The Pre-Iroquoian Algonkian Indians of Central and Western New York. (Indian Notes and Monographs, vol. II, no. 1, 37 pp.) New York: Museum of the American Indian, Heye Foundation, 1919.

——. An Ancient Algonkian Fishing Village at Cayuga, New York. (*Ibid.*, vol. II, no. 2, 43–57 pp.) 1919.

Souza, Geraldo H. de Paula. Notes sobre uma Visita a Acampamentos de Indios Coingangs. (Revista do Museu Paulista, vol. X, 1918, pp. 739–758.)

Spier, Leslie. The Growth of Porto Rican Boys with Special Reference to the Relation between their Stature and Dentition. (Journal of Dental Research, vol. I, 1919, pp. 145–157.)

Stopford, John S. B. The Variation in Distribution of the Cutaneous Nerves of the Hand and Digits. (Journal of Anatomy, vol. LIII, pt. I, Oct. 1918, pp. 14 25.)

Tauern, O. v. Patasiwa und Patalima. Vom Molukkeneiland Seran und seinen Bewohnern. 96 pls.

Teggart, Frederick John. Anthropology and History. (The Journal of Philosophy, Psychology, and Scientific Methods, vol. XVI, no. 25, Dec. 4, 1919, pp. 691–696.)

Ten Kate, Herman F. C. De Indiaan in de Letterkunde. (Over gedrukt uit "De Gids," 1919, no. 7.) 66 pp.

Thurnwald, R. Vorläufiger Bericht über Forschungen im Innern von Deutsch-Neu-Guinea in den Jahren 1913–1915. (Zeitschrift für Ethnologie, 1917, pp. 147–179, 29 ills.)

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——. Religion und Gesellschaftsordnung bei Naturvölkern. (Deutsche Literaturzeitung, 1919, no. 36, pp. 690-693.)

Walkoff, O. Die phylogenetische Entwickelung und Ausbildung des menschlichen Kinnes. (Archiv für Anthropologie, vol. XVII, 1919, pp. 12–43.)

Wallis, W. D. The Romance and the Tragedy of Sneezing. (The Scientific Monthly, Dec. 1919, pp. 526–538.)

Weise, J. Belgisch-Kongo. Geschichtliche, geographische, und volkswirtschaftliche Studie. 1916.

Woermann, K. Geschichte der Kunst aller Zeiten und Völker. 6 vols. 1915–1919.

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# DISCUSSION AND CORRESPONDENCE

## CORRECTION TO KINSHIP TERMS AMONG THE NORTHEASTERN ALGONKIAN

IN my paper "Kinship Terms and the Family Band among the Northeastern Algonkian," *American Anthropologist*, vol. XX, no. 2 (1918), some typographical errors have been called to my attention which I desire to correct. I also take occasion to add several points regarding the terms ascertained by later inquiry.

In the Malecite and Passamaquaddy columns, on page 158 there is an error of alignment involving the first eleven terms. The first term in this column *nəma'gədem* should go up one space to make it level with Micmac *nida'k'$^{cw}$*. Incidentally the corresponding term *nəda'k'$^{w}$* in Malecite does not stand as an equivalent for the Micmac *nida'k'$^{w}$* as "sister's husband," man speaking. In the Micmac column the terms in parentheses are synonymous for the name directly above them. This sets the Malecite and Micmac columns in proper alignment. Next, the first eleven terms in both these columns should go up one space to form a lateral alignment with the Abenaki and Penobscot columns. Another question raised by Dr. Sapir in correspondence with me, can be answered here. On page 157 in the Malecite column the two terms in parenthesis *nzi'wòs* and *nada'wus* refer to either cross or parallel cousins indiscriminately. They are not applied, at least nowadays, to one set of cousins more than another.

Again, on page 156, concerning the seventh and eighth Micmac terms, I inquired of Joe Toney, a Micmac, and learned that he gave *nsugwəs* as either paternal or maternal aunt. On page 160, the first term in the Malecite column should be *nəgwus* not *nŏgwus*. Also on page 156, the fifth Penobscot term, "grandfather," should be *nəmu'su'məs*. Another typographical error is on page 151, *nγmo'su'məs* should be *nəmo'su'məs*, step grandfather." I might also add the statement, which should have appeared in the discussion, that no taboo against the mother-in-law in any form has been encountered among the northeastern Algonkian. Perhaps it is well for the sake of emphasis that this statement has to be added in the corrigendum.

F. G. SPECK



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# ANTHROPOLOGY AT THE CAMBRIDGE MEETING AND PROCEEDINGS OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION

THE American Anthropological Association held its eighteenth annual meeting at the Peabody Museum, Harvard University, December 29 and 30, 1919.

Four meetings of the Council were held at which President Wissler presided:

COUNCIL MEETING, DECEMBER 29, 9:20 A.M.

The following reports were read:

REPORT OF THE SECRETARY

The Proceedings of the last annual meeting of the American Anthropological Association were published in the *American Anthropologist* for January-March, 1919. There has been no special meeting of the Association nor of the Council during the year.

The committee, consisting of Franz Boas, Aleš Hrdlička, and A. M. Tozzer, appointed by the President reported to the President of the National Research Council on March 6, 1919, regarding the work actually done by American anthropologists along the lines of research. This report was printed in the *American Anthropologist* for April-June, 1919.

The report of the election of Franz Boas, R. B. Dixon, J. Walter Fewkes, A. L. Kroeber, Berthold Laufer, and Clark Wissler as representing the Anthropological Association in the Division of Anthropology and Psychology of the National Research Council has been printed in the *American Anthropologist* for July-September, 1919. At the first meeting of the division, held in Washington, October 20, P. E. Goddard, Aleš Hrdlička, and A. M. Tozzer were also elected members.

The anthropological membership of the Division is now as follows:

To serve until July 1, 1920, Franz Boas, A. Hrdlička, Clark Wissler.

To serve until July 1, 1921, J. W. Fewkes, P. E. Goddard, A. M. Tozzer.

To serve until July 1, 1922, R. B. Dixon, A. L. Kroeber, Berthold Laufer.

The anthropological membership of the Executive Committee of the Division consists of Franz Boas and J. Walter Fewkes.

The Association has lost by death during the year seven members, Theodore de Booy, a Councillor, Mrs. Phoebe A. Hearst, a Founder,



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Captain R. G. Fuller, Dr. A. Jacobi, Rev. G. H. Jones, Professor H. Montgomery, and George E. Dimock.

Thirteen members have resigned, seven have been dropped, and 38 new names have been added to the list of members, making a net gain of twelve. The membership at present is as follows:

| | |
|---|---|
| Honorary members | 5 |
| Life members | 13 |
| Regular members | 483 |
| Total | 501 |

ALFRED M. TOZZER,
*Secretary*

## REPORT OF TREASURER

*Receipts*

| | | |
|---|---|---|
| Balance on hand, January 1, 1919 | | $ 807.49 |
| Anthropological Society of Washington | $ 200.00 | |
| American Ethnological Society | 307.13 | |
| Annual Membership dues: | | |
| 1916 | 6.00 | |
| 1917 | 12.00 | |
| 1918 | 90.00 | |
| 1919 | 1,807.03 | |
| 1920 | 339.80 | |
| Sale of publications | 191.66 | |
| Reimbursements | 515.33 | |
| Rehabilitation Fund | 485.00 | 3,953.95 |
| Total Receipts | | $4,761.44 |

*Disbursements*

| | | |
|---|---|---|
| New Era Printing Company | $3,359.87 | |
| Beck Engraving Company | 578.78 | |
| Editor's and Treasurer's and Secretary's Expenses | 503.49 | |
| Total Disbursements | 4,442.14 | |
| Cash on hand | 319.30 | |
| | $4,761.44 | $4,761.44 |

*Resources*

| | | |
|---|---|---|
| Cash on hand, December 22, 1919 | | $ 319.30 |
| Due from the Anthropological Society of Washington | $100.20 | |
| Due from engravings | 45.22 | |
| Due from sales | 19.61 | |
| Due from dues (1917, 1918, 1919) | 349.00 | 514.03 |
| Total Resources | | $833.33 |

*Liabilities*

| | | |
|---|---|---|
| Owing New Era Printing Company (bill of 12-20-19) | 498.88 | |
| Membership dues for 1920 already paid | 339.80 | |
| Total Liabilities | $838.68 | |
| Net Indebtedness | 5.35 | |
| | $833.33 | $833.33 |

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*Cost of Publications*

| | | | |
|---|---|---|---|
| *American Anthropologist*, vol. 20, no. 4: | | | |
| Engravings | $ 95.58 | | |
| Printing | 261.76 | $357.34 | |
| Reimbursements | | 79.57 | |
| Net Cost | | | $ 277.77 |
| *American Anthropologist*, vol. 21, no. 1: | | | |
| Engravings | $ 68.82 | | |
| Printing | 358.85 | $427.67 | |
| Reimbursements | | 53.96 | |
| Net Cost | | | $ 373.71 |
| *American Anthropologist*, vol. 21, no. 2: | | | |
| Engravings | $ 38.86 | | |
| Printing | 339.79 | $378.65 | |
| Reimbursements | | 33.98 | |
| Net Cost | | | $ 344.67 |
| *American Anthropologist*, vol. 21, no. 3: | | | |
| Engravings | $ 24.45 | | |
| Printing | 411.12 | $435.57 | |
| Reimbursements | | 15.64 | 419.93 |
| Net Cost | | | $1,416.08 |
| *Memoirs*, vol. 5, no. 4: | | | |
| Engravings | $ 42.15 | | |
| Printing | 55.36 | $ 97.51 | |
| Reimbursements | | 42.15 | |
| Net Cost | | | $ 55.36 |
| *Memoirs*, vol. 5, index: | | | |
| Printing | 28.13 | | 28.13 |
| *Memoirs*, vol. 6, no. 1: | | | |
| Engravings | $ 28.55 | | |
| Printing | 181.03 | $209.58 | |
| Reimbursements | | 28.55 | |
| Net Cost | | | 181.03 |
| *Memoirs*, vol. 6, no. 2: | | | |
| Engravings | 3.86 | | |
| Printing | 213.43 | $217.29 | $ 217.29 |
| Net Cost | | | $ 481.81 |
| *American Anthropologist*, net cost | $1,416.08 | | |
| *Memoirs*, net cost | 481.81 | | |
| | | | 1,897.89 |
| Reprints and distribution. | | | |
| *American Anthropologist*, vol. 20, no. 4 | 48.43 | | |
| *Memoirs*, vol. 5, no. 4; *Memoirs*, vol. 6, no. 1 | 85.63 | | |
| *American Anthropologist*, vol. 21, no. 1; *American Anthropologist*, vol. 21, no. 2; *American Anthropologist*, vol. 21, no. 3; *Memoirs*, vol. 6, no. 2 | 175.69 | | $309.75 |
| Total Cost | | | $2,207.64 |

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PERMANENT FUND

*Receipts*

| | | | |
|---|---|---|---|
| Balance, Jan. 1, 1919 | | $1,304.00 | |
| Interest, May 29 | $ 5.36 | | |
| Interest, Oct. 15 | 4.26 | $9.62 | |
| Total Receipts | | | $1,313.62 |

*Disbursements*

| | | | |
|---|---|---|---|
| Liberty bonds, June 10 | $190.90 | | |
| W. S. S., June 11 | 16.68 | | |
| W. S. S., Oct. 21 | 4.21 | | |
| Cash in envelope | 1.83 | $213.62 | |
| Loan to General Fund | | 1,100.00 | |
| Total disbursements | | | $1,313.62 |

The accounts of the Treasurer, P. E. Goddard, have been examined and found correct:

Signed:

B. T. B. HYDE,
GEORGE H. PEPPER,
*Auditing Committee*

Since the income, past and prospective, actually attributable to the year 1919 is not shown in the above statement the following is submitted:

| | | | |
|---|---|---|---|
| Annual dues for 1919 paid in 1918 | | $ 161.85 | |
| Annual dues for 1919 paid in 1919 | | 1,807.03 | |
| Annual dues for 1919 still due | | 252.00 | $2,220.88 |
| Publications furnished: | | | |
| American Ethnological Society | | 307.13 | |
| Anthropological Society of Washington | | 136.50 | $ 443.63 |
| Sales: | | | |
| Collected | | 191.66 | |
| Still due | | 19.61 | 211.27 |
| | | | $2,875.78 |

This amount furnishes a basis for estimating the probable income in 1920 based on which a budget can be adopted.

According to the statement furnished last year the Association had a net indebtedness of $889.17. This statement did not take into account an amount of $163.70 due the Association or the membership dues uncollected. The latter item was practically offset by the dues collected in 1918 for the year 1919 which are in reality a liability. If the statement of the indebtedness for 1918 be corrected by deducting $163.70 and adding an error of $2.87 in the statement of last year, the corrected

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deficit existing January 1, 1919, is $727.34. This indebtedness of the Association has been reduced by $711.32 leaving a deficit of $5.35. This result was accomplished by gifts amounting to $485 and by an excess of normal income over expenses of $236.99. The following persons contributed to the Rehabiltation Fund:

J. B. Bliss
C. P. Bowditch
C. L. Hay
W. H. Holmes
E. Lindsey
S. K. Lothrop
R. H. Lowie
G. G. MacCurdy
C. B. Moore
E. C. Parsons
T. M. Prudden
H. E. Sargent
F. G. Speck
H. J. Spinden
A. M. Tozzer
G. F. Will
J. H. Wilson
C. Wissler
F. A. Woods

Respectfully submitted,

P. E. GODDARD,
*Acting Treasurer*

## REPORT OF THE EDITOR

THE editor was allowed for the purposes of publication during the year the sum of $2,000. The expenses for which bills have been received to date amount to $2,207.64 of which amount $24.45 will be reimbursed by authors. This sum covers the cost of four numbers of the *American Anthropologist* and three numbers of the *Memoirs*. The fourth number of the *Memoirs* (volume 6, number 3) will contain about one hundred pages including two color plates. The paper is entitled "Notes on Cochiti" by Father Nöel Dumarest, edited by Elsie Clews Parsons. The color plates, ready printed, and the many engravings have been provided by the editor of the number. The printing costs will be about $300.

It was not the editor's intention to overstep the allowance of the budget but, when practically all the matter so far in type was in the hands of the printer, our printers on August 1st notified the editor that the rates would be advanced as of that date. The scale of new charges was not received until November 6. The rates had not been increased since 1903 when the New Era Printing Company became our publishers. During the last two years however, an additional charge has been made to cover the advance in the cost of paper. The new rates for composition and printing have been increased a little more than 50 per cent. A charge for the binding, which used to be included

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in the page rate, has been added. This latter charge about offsets the charge for paper mentioned above. Except for an increase in the printing and binding of plates, the Association faces an increase of 50 per cent in the cost of issuing its publications. On the average this will mean a cost of about $4 per page. As far as possible plates must be avoided or paid for by the authors of the articles. The additional charges for binding applies this year to three numbers of the *Anthropologist* and to one *Memoir*, and the advanced composition rates to two numbers of the *Anthropologist* and one *Memoir*. This additional and unexpected increase in cost has betrayed the editor into exceeding his budget by the amount mentioned above.

For the year 1920 it is recommended that $2,000 be allowed for publishing the *American Anthropologist*. This will permit a volume of about 475 pages. We are under obligations to complete volume VI of the *Memoirs*, the fourth number of which is now in the hands of the printer. This is a paper on "Penobscot Shamanism" by Frank G. Speck and will make about forty pages costing $160. An index for the volume will cost about $40. To complete volume VI then will require $500. To meet this charge there will be available $250 from our regular income. The additional amount required should be provided in some manner.

The question whether the Editor shall be authorized to continue with the *Memoirs* and issue during 1920 two or three numbers of volume VII should be determined by the Council.

Respectfully submitted,

P. E. GODDARD,

*Editor*

The President appointed Messrs. B. T. B. Hyde and George H. Pepper to audit the accounts of the Treasurer.

It was resolved by motions duly made and passed:

That in the future it shall be the policy of the Association to elect the same individual to the office of Editor and Treasurer.

That the present membership of the Executive Committee be maintained by the election of one additional member to fill the place left vacant by this consolidation of offices.

That the budget for the year 1920 be:

$2,000 to print the *American Anthropologist*
600 for office expenses
200 to complete volume VI of the *Memoirs*.

That the Chairman appoint a committee with power to arrange for

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increasing the charge to the Anthropological Society of Washington and the American Ethnological Society for the *American Anthropologist* from $3.50 to $4.00 per volume.

That it is the sense of the Association that the *Memoirs* be continued if possible.

That the publication of the *Memoirs*, other than volume VI, be referred to the Executive Committee with power to act.

The President appointed as nominating committee Mr. Hodge, Dr. Fewkes, and Dr. Kroeber.

The Council adjourned at 10:20 a.m.

### Council Meeting, December 30, 9:15 a.m.

The committee on nominations made the following report:

*President:* Clark Wissler.

*Vice-President:* 1923, J. R. Swanton.

*Secretary:* A. M. Tozzer.

*Treasurer-Editor:* P. E. Goddard.

*Associate Editors:* J. R. Swanton, R. H. Lowie.

*Executive Committee:* E. Sapir, F. G. Speck, J. W. Fewkes, R. B. Dixon.

*Council:* A. E. Jenks, S. A. Barrett, W. Hough, N. M. Judd, B. T. B. Hyde, F. G. Speck, A. A. Goldenweiser, E. A. Hooton, A. V. Kidder, F. C. Cole, L. Spier, R. B. Bean, H. H. Wilder, S. G. Morley, G. H. Pepper (1923); G. B. Gordon, T. T. Waterman (1922); L. W. Jenkins, C. E. Guthe, J. P. Harrington (1921); W. E. Gates, M. R. Harrington, S. J. Guernsey (1920).

*Members of the Division of Anthropology of the National Research Council to be effective at the close of the present fiscal year:* Clark Wissler, F. W. Hodge.

*Delegates of the Association to the Council of A.A.A.S.:* Clark Wissler, J. W. Fewkes.

The Council adjourned at 9:30 a.m.

### Annual Meeting of the Association, December 30, 4:00 p.m.

The officers and members of the Council as nominated were duly declared elected by a vote ordered cast by the Secretary. The following members were also elected to membership in the Association:

| | |
|---|---|
| Miss Nellie Barnes | "Archaeology" (Japan) |
| George L. Collie | C. H. Danforth |
| Miss Leona Cope | S. J. Guernsey |
| Lamar Crawford | Miss Erna C. Gunther |

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A. Irving Hallowell
Hamilton College Library
S. W. Heavenrich
Hobart College Library
Capt. A. de Hostos
W. W. Hyde
J. A. Jeancon
W. B. Kirkham
C. M. McLean
North Dakota State Hist. Society
W. F. Ogburn
Oregon State Library,
Frank Pinkley
Queen's University Library
Talbert F. Reavis
Miss Gladys A. Reichard
B. F. Schappelle
Hyman Schor
Sociedad Cientifica "Antonio Alzate"
V. Stefansson
Chester E. Story
Toledo Public Library
University of the South Library

The time and place of the next annual meeting was referred to the Executive Committee with power.

The Chairman was instructed to appoint a committee to draft resolutions of thanks to the local members in Cambridge for their hospitality.

The Chairman was authorized to appoint a committee of three members to act in coöperation with a similar committee of the Archaeological Institute to inquire into the feasibility of the proposed prehistoric foundation in France, and in case of an affirmative judgment this committee has the approval of the Association to continue its activities.

President Wissler appointed Dr. Peabody (chairman), Professor MacCurdy, and Dr. Hrdlička.

It was directed that Dr. Wissler and Dr. Fewkes, the two members of the Executive Committee of the National Research Council, select a third member of the American Anthropological Association to act with the National Research Council and that they be authorized to consider certain proposals from Professor Moorehead concerning archaeological conditions in Maine and Illinois.

### Council Meeting, December 30, 4:45 P.M.

Resolutions received from the Anthropological Society of Washington were read and on motion accepted and placed on file.

The following resolution was moved by Neil M. Judd:

"*Resolved*: That the expression of opinion by Dr. Franz Boas contained in an open letter to the editor of *The Nation* under date of October 16, 1919, and published in the issue of that weekly for December 20, 1919, is unjustified and does not represent the opinion of the American Anthropological Association. Be it further resolved:

"That a copy of this resolution be forwarded to the Executive Board

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of the National Research Council and such other scientific associations as may have taken action on this matter."

Yes: Judd, Hyde, Hooton, Kidder, Wilder, Farabee, Spinden, Hagar, Wardle, Lothrop, Saville, Fewkes, MacCurdy, Gates, Guernsey, Guthe, Gordon, Dixon, Hodge, Morley.

No: Speck, Spier, Kroeber, Peabody, Sullivan, Lowie, Nelson, Parsons, Tozzer, Goddard.

Not voting: Willoughby, Wissler (presiding).

The Council adjourned at 6 p.m.

## Addresses and Papers

The following papers were presented:

*Monday, December, 29, 11 a.m.*

Note of Welcome: Charles C. Willoughby, Cambridge.

The Jaguar and Serpent Mural at Chichen Itza: Stansbury Hagar, New York.

The Marshall Islands Charts: H. U. Hall, Philadelphia.

Shark's Teeth Weapons of the Pacific: H. Newell Wardle, Philadelphia.

An Aboriginal Cornfield in Northampton, Massachusetts: Harris H. Wilder, Northampton.

Bronze Arrow-heads recently found in Connecticut and their probable Norse Origin: Neil M. Judd, Washington, D. C.

*2.30 p.m.*

Review of recent Archaeological Investigations in Western Utah: N. M. Judd, Washington, D. C.

Notes on the Excavation of open Pueblo Ruins: Alfred V. Kidder, Andover and Cambridge.

Small Surface Ruins in the Mesa Verde National Park: R. Linton, Cambridge.

The Wawenock, a new Wabanaki Dialect: Frank G. Speck, Philadelphia.

The Status of Linguistic Classification in Aboriginal North America: Edward Sapir, Ottawa. (Read by title.)

Is Chinook an Isolated Linguistic Stock? Edward Sapir, Ottawa. (Read by title.)

The Problem of Genetic Relationship of Languages: Paul Radin, Berkeley, California. (Presented by Kroeber.)

Physical Differences between Savannah and Forest Tribes belonging to the same Stock: William C. Farabee, Philadelphia.

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Aspects of the Skull. How shall they be represented?: GEORGE G. MACCURDY, New Haven.

Physical Types of the California Indians: LEONA COPE, Berkeley, California. (Read by title.)

Notes on the care of Skeletons in the Field: CARL E. GUTHE, Andover.

Notes on the Sitting Height in Man: ROBERT B. BEAN, University, Virginia. (Read by title.)

A Maya Manuscript Ritual of the Bacabs: WILLIAM E. GATES, Baltimore.

The Correlation of Central American Calendars and the Gregorian Day: HERBERT J. SPINDEN, New York.

*10 a.m. American Folk-Lore Society*

The Study of Variants: Presidential Address, ELSIE CLEWS PARSONS.

The Importance of Music in the Study of Ballad Origins: PHILLIPS BARRY, Cambridge.

A little experience in folk-lore in Kentucky: MISS MARGARET PEABODY.

The Culture Area Concept: A. A. GOLDENWEISER, New York.

The Diffusion of the Sun Dance: LESLIE SPIER, New York.

The Age of Kechipauan, one of the Seven Cities of Cibola: F. W. HODGE, New York.

The Earliest Notices concerning the Conquest of Mexico by Cortes in 1519: MARSHALL H. SAVILLE, New York.

Archaeological Investigations in Southwestern Pennsylvania: WILLIAM C. FARABEE, Philadelphia.

Riddle Forms among the Jamaica Negroes: MARTHA W. BECKWITH, Northampton. (Read by title.)

On the Principle of Order in Civilization as exemplified by Changes in Fashion: ALFRED L. KROEBER, Berkeley. (Read by title.)

Aboriginal Fish Nets in North America: A. I. HALLOWELL, Philadelphia. (Read by title.)

*2.30 p.m.*

Joint meeting with the American Psychological Association at Emerson Hall, Room J.

The National Research Council: JAMES R. ANGELL, Chicago. (Read by W. D. Scott.)

Enterprises in Anthropological Research which present Opportunities for Coördination and Coöperation: CLARK WISSLER, New York.

Aims and Problems of the Division of Anthropology and Psychology of the National Research Council: WILLIAM V. BINGHAM, Pittsburgh.

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*4:00 p.m.*

A Proposed Foundation for the Encouragement of Prehistoric Studies: CHARLES PEABODY, Cambridge.

Dangers of Archaeological Legislation: WARREN K. MOOREHEAD, Andover.

Suggestions for Coöperation in Field Work among Scientific Institutions: WARREN K. MOOREHEAD.

*Wednesday, December 31*

The members enjoyed a trip to the Peabody Museum at Salem and to the Museum at Phillip's Academy, Andover, by invitation of the officers of these institutions.

ALFRED M. TOZZER,
*Secretary*

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# ANTHROPOLOGICAL NOTES

ERNEST VOLK, an archaeologist of note, died as the result of an automobile accident, September 15, 1919. He was born in Waldkirch, Germany, August 25, 1845, but spent his adult life at Trenton, New Jersey. In 1899 he became associated with Frederick W. Putnam and continued to work under his direction for many years. He gave his whole attention to the question of man's antiquity as shown in the deposits near Abbott's home at Trenton. In 1911 he published a full report of twenty-two years of research at Trenton, *The Archaeology of the Delaware Valley* (Papers of the Peabody Museum of American Archaeology and Ethnology). In this work he sought to establish three levels of culture: 1, in the black soil; 2, the yellow drift; and 3, glacial gravel. These had been proposed by Abbott in 1883 but were not supported by extensive excavations as in the case of Volk, who must be regarded as the real investigator of the problem.

Mr. Volk was a man of fine character, a naturalist of the old school who loved his work and took infinite pains with the smallest details. His long and patient search for traces of man in the glacial gravels at Trenton is one of the most human touches in the story of American archaeology, intensified by the tragic interruption of his great task. The extracts from his diary, found in the publication cited above, stand as a unique contribution to the literature of American anthropology and will be read with delight by the experienced collectors of the future.

FROM *Petermann's Mitteilungen*, May-June 1919 issue, we learn that the following courses with anthropological bearing are to be offered at the new University of Hamburg, each lecturer having the status of a professor extra-ordinarius: Karl Florenz, Japanese language and culture; Otto Franke, Chinese language and culture; Sten Know, History and culture of India; Karl Meinhof, African languages; Georg Thilenius, Physical anthropology and ethnology; Siegfried Passarge Geography.

On December 29, 1919, Dr. Otto Stoll, professor emeritus of the University of Zurich, celebrated his seventieth birthday. He has been known to Americanists for his Central American researches and for his book on hypnotism among the ruder peoples of the globe.

A movement is on foot for the introduction of teaching on anthrological subjects into the curriculum of the University of New Zealand,



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and many of the leading anthropologists of Great Britain have made strong representations to the Registrar of the University on its behalf. An editorial on the subject accompanied by six letters from British anthropologists is contained in *The New Zealand Journal of Science and Technology*, vol. 1, no. 5, pp. 257–264.

Dr. Truman Michelson of the Bureau of American Ethnology has been elected an Associate Member of the Societe des Americanistes de Paris.

AT a meeting of the Division of Anthropology and Psychology of the National Research Council held at Harvard Union, Cambridge, Mass., December 30, 1919, Professor Franz Boas presented his resignation as a member of the Division. The following resolution was unanimously adopted:

That the Chairman be instructed to transmit to the Executive Board of the National Research Council the resignation of Professor Boas, with regret that the Division must lose the services of the most eminent anthropologist in America and a man of unimpeachable devotion to his ideals, and with the recommendation that Professor Boas' resignation be accepted.

IN the "Anthropological Notes" in the issue for July-September, 1919, attention is called to the coincident chewing of vegetable substances by the Peruvian Indians on the one hand, by the Melanesians on the other, in both cases with the admixture of lime. There are at least two other such coincidences to which attention has not been called, as far as I am aware. In "Melanesians and Polynesians," by Brown (Macmillan, 1910), there is a description of trephining as practised by certain Melanesians, and the statement is made that use was made of knotted strings as mnemonic aids.

H. E. EGGERS, M. D., Univ. of Nebraska.

THE present day practical application of anthropological knowledge is seen in the recognition of Anthropology in the leadership of the new wide-spread endeavors in the Americanization field. Dr. Albert E. Jenks, Professor of Anthropology of the University of Minnesota, was elected President of the National Council of Americanization Workers at an organization meeting of Americanization Directors, Teachers, Supervisors, etc., held in Cleveland, Ohio, February 24th and 25th, at the call of United States Commissioner of Education, Philand H. Claxton.

The first national meeting of the Council will be in Minneapolis, May 31 and June 1, 1920. In 1918 the University of Minnesota estab-

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lished the first scientific training course in the Americanization field. The course grants a B.S. degree, and is under the directorship of Professor Jenks, the President of the new National Council.

MR. REGINALD PELHAM BOLTON has nearly completed a manuscript dealing with the aboriginal occupation of the metropolitan district of New York, based on examination of the original deeds covering the sale of land in this vicinity. New and interesting light is thrown on the tribal affiliations of these Indians, which independently confirms the conclusions arrived at from archaeological explorations recently conducted in this vicinity. Mr. Bolton's paper will appear in the near future in "Indian Notes and Monographs" of the Museum of the American Indian, Heye Foundation.

As the result of excavations and explorations in the neighborhood of Green Bay, Wisconsin, Mr. J. P. Schumacher, of that city, has acquired a number of typical Iroquois pipes and pottery fragments. The origin of these pieces can be accounted for by the fact that there were a number of settlements of Huron fugitives on Green Bay, following the downfall of that tribe at the hands of the Five Nations, during the middle and latter part of the seventeenth century.

MR. M. R. Harrington, of the Museum of the American Indian, Heye Foundation, is engaged in preparing for publication, the results of his archaeological explorations in Cuba, beginning in 1915, which lasted more than a year, and covered mainly the district about Cabo Maisi at the eastern extremity of the island. This publication will also include his trip in the spring of 1919, during which time he made some interesting archaeological discoveries in the hitherto unknown caves and refuse heaps, near Cabo San Antonio, at the western end of Cuba. He also secured some very unusual wooden specimens from the muck of a lake bed in the same region.

MR. LOUIS R. SULLIVAN of the American Museum of Natural History, left in April to undertake joint work for that Museum and the Bishop Museum of Ethnology and Natural History, Honolulu, H. I. A study is to be made of the physical anthropology of the inhabitants of the Hawaiian Islands. The work is expected to consume at least a year.

THE death of E. O. Randall, Secretary and Editor of the Ohio Archaeological and Historical Society of Columbus, Ohio, has been announced.

MR. C. B. GALBREATH, former State Librarian of Ohio, has been appointed Secretary of the Ohio State Archaeological and Historical Society to fill the vacancy created by the death of Mr. E. O. Randall.

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# THE SOCIAL ORGANIZATION OF THE KWAKIUTL

BY FRANZ BOAS

IN the Annual Report of the United States National Museum for 1895 I have given a description of the social organization and secret societies of the Kwakiutl based on observations and inquiries made prior to 1895. Further information relating to the social organization of the Kwakiutl collected on my last visit to Vancouver island, and since that time obtained through correspondence with Mr. George Hunt clears up a number of points of this difficult problem.

One of the greatest obstacles to a clear understanding of the social organization of the Kwakiutl is the general confusion caused by the reduction in numbers of the tribe. I have tried to clear up the situation by recording the histories of a number of families in all possible detail. In the following I shall give the principal results that may be derived from my collection of data.

I will begin with the discussion of what constitutes a tribe. There is a very fundamental difficulty in the definition of the tribal unit and of its subdivisions. I do not know of a single Kwakiutl tribe that is at present an undivided unit. All those studied consist of well-recognized subdivisions.

Furthermore, a single locality is claimed as the place of origin of each division of the tribe. In the consciousness of the people these divisions are fundamental units. The development of the concept of a tribal unit is not, by any means clear, except in so far as it appears as an effect of the congregation at one place of a number of local units. Recent tradition, the historical truth of



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which cannot well be doubted, shows clearly that such a congregation has occurred repeatedly. Units may also have broken up, owing to inner dissensions or to other accidents.

On the other hand, each tribe consists of units that claim as their places of origin, localities not far apart. In a few cases only, may one or the other division of the tribe claim as the place of its origin a locality removed quite a long distance from the traditional home of the other divisions. This is the case for instance with the Q!ōmk'!ōt!Es of the Kwāg'uł. Some of the tribal names are purely geographical terms and indicate that we are dealing with communities that live in close proximity, including perhaps groups that moved to the territory in question. Other types of names, however, occur. The translations given by the natives for some of them are folk-etymologies and cannot be taken as authoritative. Thus the name Kwāg'uł is derived from a stem *kwak*$^{u}$- of unknown significance, but is considered by the natives as a derived form of *kwax*'—which means "smoke." The name Nāk!wax'da'x$^{u}$ is explained by them as derived from *nEq*-, "ten," philologically an impossible etymology. In previous writings, I accepted some of these etymologies, but I am certain that they must be rejected.

There are a number of cases in which the relations between certain divisions of a tribe are explained by tradition. Thus several pairs of divisions of one sept[1] of the Kwāg'uł are considered as the descendants of two brothers, one of the elder, the other of the younger one. In another case, the divisions of the tribe are considered each as descended from one of four brothers. When I inquired later on why in one of these pairs the one division was considered of lower rank, the following information was obtained. In the generation *I* the ancestor of one division *a* of one sept *A* had a slave whom we may call *IAa*1. He married the woman slave of the ancestor of another division *b* of another sept *B* whom we may call *IBb*2. Their eldest son (Generation *II*, designated *IIBb'*3), married the daughter (*IIAc*7) of the chief (*IAc*6) of the division *c* of the sept *A*, assumed a chief's name and became the ancestor of

[1] The Kwāg'uł proper consist of four septs or subtribes, each being divided into a number of subdivisions which are the fundamental social units.

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the division *b'*, of the sept *B*, or of the line *Bb'*, which is up to this time associated with *Bb*. At a former time this line was described to me as descended from the younger brother of the ancestor of *Bb*. The daughter of *IAa*1 and *IBb*2, whom we may call *IIBb'*5 married the fifth son (*IICd*11) of the chief (*ICd*10) of the division *d* of the sept *C*, whom we may call *IICd*10. Their daughter (*IIICd'*13) married her father's eldest brother *IICd*14, without letting him know of her descent. Therefore her descendants were not accepted by her husband's division *Cd*, but assigned to *Cd'*. The elder son (*IIICd'*15) of this couple married the daughter (*IIIBb'*8) of the couple who had established the line *Bb'*, and their children also belong to the line *Cd'*. Their descendants are the division *Cd'*, which is up to this day associated with the line *Cd*. These relations

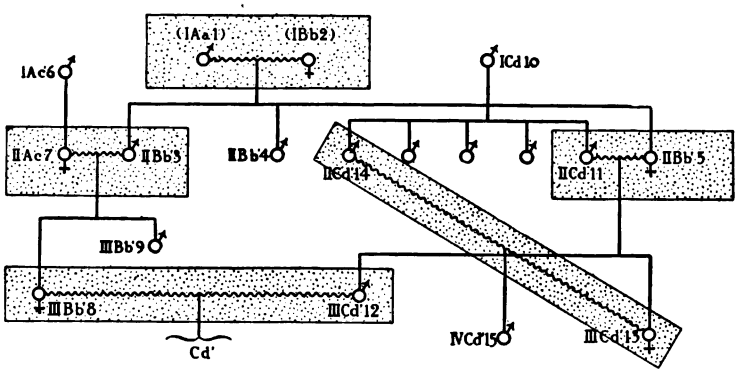



FIG. 3.—(P. 1094.) 1. LIÕLlotsa, slave of ʿwālas Kwax·ilanōkumēʿ; named NenōlogEmēʿ by Lālax·sʿEndayo with whom he lived after his marriage to No. 2. 2. TsēlĒ, slave of Lālax·sʿ Endayo; named Lālax·sʿaqlanakᵘ by Lālax·sʿ Endayo. 3. Llēsplegaakᵘ (name given by Lālax·sʿ Endayo); gives to himself the name G·ēxk·Enis and goes to live with DzEnx·qlayo. Numaym: Elgūnwēʿ. 4. BawElē (name given by Lālax·sʿ Endayo). Numaym: Elgūnwēʿ. 5. Alāk·ilayugwa (name given by Lālax·sʿ-Endayo). Numaym: Elgūnwēʿ. 6. DzEnx·qlayo; Hayałk·EngEmēʿ. 7. EzEnx·qlayugwa. 8. Alāg·îmīł. Numaym: Elgūnwēʿ. 9. Ek·lawig·iʿlakᵘ Numaym: Elgūnwēʿ. 10. HäxŭyōsEmēʿ Numaym: Haăyalik·awēʿ; Tribe: Qlōmoyâʿyē. 11. Tslâg·iʿlakᵘ Numaym: Haăyalik·awēʿ; Tribe: Qlōmoyâʿyē. 12. MâʿnakŭIa Numaym: Lâxsä. 13. AlēʿstalidzEmga Numaym: Lâxsä. 14. ʿmāxŭyalisEmēʿ Numaym: Haăyalik·awēʿ; Tribe: Qlōmoyâʿyē. 15. ʿmāxuyalisEmēʿ Numaym: Lâxsä.

are illustrated by the following diagram (fig. 3). This is an example of the intricate mythological interrelations between the divisions that belong to a single tribe.

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On the other hand it is claimed that two divisions of one sept of the Kwāg·uł which are assumed to be descended from two brothers and whose names are found among many tribal groups, were scattered among the different tribes. Since their names are honorific names (G·iġ·îlgăm, *i.e.*, the first ones and G·ēxsEm, *i. e.*, chief's group), it may be doubted whether any historic meaning attaches to this tradition. This is more plausible for the division Kŭkwāk!ŭm which is found among two septs of the Kwāg·uł, which, according to tradition, are assumed to be derived from the same place of origin. In some cases we find in a tribe a subdivision which has for its name the stem of the tribal name with the ending *-Em*, as in the division just mentioned, the SēnL!Em and lēq!Em and outside of the Kwāg·uł proper, the Mamalēleq!am. The meaning of this ending is "the real ones." According to the statement of the Indians there was, in former times, in almost each division a noble family that bore a name of this type, while the rest of the people were designated by the ordinary name of the division. Mythologically this is explained as meaning that the select group, called "the real" members of the division, were descended from the ancestor, while the other families at an early time became associated with the ancestor without being descended from him.

We may therefore say that in the concept of the Indians, the tribe consists of a number of divisions, each of which is derived from one ancestor, but which includes also individuals of different descent who at an early time joined the ancestor. In a number of cases, the ancestors of the various divisions are brothers and the divisions represent elder and younger lines. In other cases there is no such relation, the lines representing disconnected local groups.

Although in the present period the concept of the tribe is very clear in the minds of the Indians, there seems to be little doubt that the tribes have undergone many changes in number and composition. There are some indications of this process even at the present time. Thus one sept of Kwāg·uł proper (the Q!ōmk·!ōt!Es) are generally grouped with the Walas Kwāg·uł, and the tendency is such that within a short time the consciousness of their separate existence

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might well disappear. The union of LˈaLˈasiqwăla and of NaqEm-gˈîlisăla in one village has not yet led to their fusion, but externally at least they form a single tribe. The stability of tribes is primarily due to the fact that the tribal units have fairly definite functions distinct from the functions of the tribal divisions. These appear particularly in formal gatherings in which the tribes are arranged in rank and in which, furthermore, definite tribes are matched. Thus in northern Vancouver island, we find the following parallel arrangement of Kwāgˈuł tribes and of the tribes further to the east.

GwētEla matched with Maˈmalēleqala
Q!ōmoyâˈyē matched with Qwēqˣsotˈlēnoxˣ
ˈwalas Kwāgˈuł matched with Nimkish
Q!ōmkˈˈlōtEs matched with Łāwitsˈlēs

Notwithstanding the relative stability of the tribes, the tribal divisions must be considered as the fundamental units. In previous writings I have used the terms "gens" and "clan" according to the varying impression of prevalence of maternal and paternal descent, both of which are important. After much hesitation I have decided to use the native term *numaym* (ˈnEˈmēˈm) because the characteristics of the unit are so peculiar that the terms "gens" or "clan" or even "sib"[1] would be misleading. We have to recognize first of all that positions in a *numaym*, or at least the ranking positions must be filled and that their disappearance, according to the ideas of the Indians, would be a misfortune. A position is defined by the name attaching to it and by a number of privileges (kˈˈlēsˈō). I prefer the term "privilege" to the term "crest," because the privileges are quite varied in character, although not so much varied in form as among the Nootka.

A clear understanding of the constitution of the *numaym* is made very difficult by the fact that the number of positions is at present greater than the number of members of the tribe, so that many individuals hold more than one position in more than one *numaym*. It may be that even in early times, important personages had the right to do so, but the present extension of this

[1] See Robert H. Lowie, "The Matrilineal Complex," *University of California Publications in American Archaeology and Ethnology*, vol. VI, vol. II, p. 30.

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right is, no doubt, due to the reduction in the number of members of the tribe. As a matter of fact, the Indians themselves are not by any means clear as to the rights of each individual, and quarrels regarding rank and position are of common occurrence. In these each party tries to defend its rights by facts based on descent. The fundamental principle seems to be that primogeniture, regardless of sex, entitles the first-born child to the highest rank held by one of its parents. Rank is, on the whole, determined by the order of birth, and the noblest line is the line of the first-born. The lowest in rank that of the youngest born. Hence when a father and mother are of equally high rank, the first-born child may be assigned to one *numaym*, the following to another *numaym*. In cases of equal rank of both parents the father's *numaym* has preference and to it the first-born child is assigned. I have never been able to learn definitely whether a child that is assigned to another *numaym*,—not his mother's—retains, nevertheless, the right to membership in his father's *numaym* or not. In some cases it seems that way, in others it seems that a person either has no position in the father's *numaym*, or that he definitely severs his relations with it and gives up his place in it. The Indians emphasize again and again the rule that the "house name" and the attached position and privileges can never go out of the line of primogeniture and may not be given away in marriage. The first-born child must take them no matter whether it is male or female. It is not clear, however, even from the genealogies at my disposal, what was done in former times if the parents did not hold enough seats and names to go around among their children, unless in these cases the children received names from the mother's father. At present and for about seventy years past, this condition has probably never arisen. The inference from the general point of view of the modern Indian is that the younger lines had names of inferior rank and formed the lower classes.

It seems to me that the conditions among the Kwāg·uł and the Nootka must have been quite similar in so far as a sharp line between nobility and common people did not exist, that rank was rather determined by the seniority of the lines of descent. In one

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Kwāg·uł tale, it is even stated that the youngest of five brothers "was not taken care of by his father and was like a slave or a dog."[1]

In case of the death of the eldest child, the younger brothers and sisters rank in order of their birth regardless of sex. Where there are no children, the younger brothers and sisters of the deceased in order of birth would be the successors to his position. When there are no brothers or sisters, a father's (or mother's as the case may be) brother and sister and their descendants would be the successors.

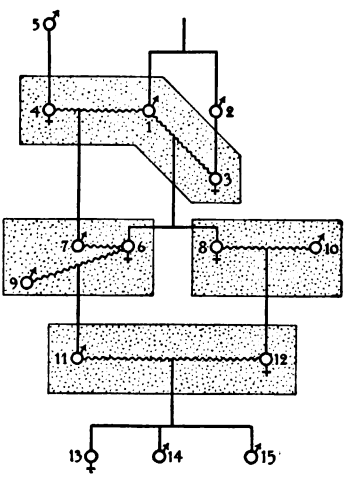



FIG. 4.—(P. 781.) 1. LālelīLla, Numaym: DzēdzEmēleqāla, Tribe: Nāk!wax·da$^{e}$x$^{u}$. 2. YaxLEn. 3. HāmēLas. 4. XwēlagēLas. 5. Hayalk·En, Numaym: SēsEnLlē$^{e}$, Tribe: Gwa$^{e}$sEla. 6. LlāqwagilayugwA. 7. Sēwid. 8. LlāłAga. 9. HēłāmAs, Numaym: G·ēxsEm, Tribe: Nāk!wax·da$^{e}$x$^{u}$. 10. G·ēxk·Endzē, Numaym: SēsEnLle$^{e}$, Tribe: Nāk!wax·da$^{e}$x$^{u}$. 11. HēlāmAs, Numaym: G·ēxsEm, Tribe: Nāk!wax·da$^{e}$x$^{u}$. 12. HāyōsdēsElas. 13, 14 died as children. 15. K·ēnē.

Among some of the noble families, we find a strong desire to retain the privileges in the narrowest limits of the family. This is done by means of endogamous marriages. Marriages are permitted between half-brother and half-sister, *i.e.*, between children of one father, but of two mothers, not *vice versa*; or, marriages between a man and his younger brother's daughter, but not with his elder brother's daughter, who is, of course, of higher rank, being in the line of primo-geniture or at least nearer to it. An excellent example is the genealogy represented in fig. 4. We have here first a marriage between a man 1 and his younger brother's daughter 3. Then a marriage between half-brother and sister 6 and 7 and finally between the son 11 and daughter 12 of two full sisters 6 and 8. It is expressly stated that these marriages were intended to prevent the privileges from going out of the family. In other genealogies I have found practically no cases of endogamy. On the contrary, we find, so far as I can see, only exogamous marriages.

[1] Franz Boas, "Ethnology of the Kwakiutl," *Thirty-fifth Annual Report of the Bureau of American Ethnology.*

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We may say that the *numayms* are based on descent with a preference for the paternal line; the highest positions in the *numaym* which form the nobility are the senior lines, at the head of which stands the first-born line. There are, therefore, a series of noble names in each *numaym* that may be considered as similar to offices which must be filled. The occupants of these positions must have the hereditary right to occupy their places, but their positions are actually determined by assignment, each occupant of a position having the right to determine his successor providing the laws of descent give him a title to the position.

The peculiar transfer of name, position and privileges from the woman's father to his son-in-law has been described by me before. The complex rules of this transfer have given rise to much discussion. Ordinarily name and position are given by a man to his son-in-law's children. This does not entail any difficulties when the woman is a first-born child and nobler than her husband, or when younger children are concerned. When the husband is the nobler, it would however, contradict the rules of primo-geniture previously described.

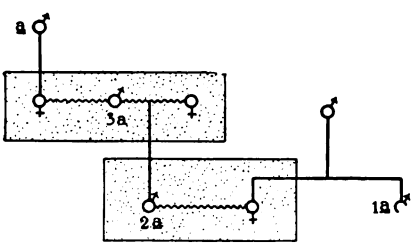



FIG. 5.—(P. 10.)

I have said in earlier publications that the son-in-law holds the name and privileges which he receives from his father-in-law on behalf of his son who becomes the real owner when he grows up. I believe this does not quite correspond to the actual conditions. In return for the marriage presents given by the young man, the father-in-law promises to give names, positions and privileges to any member of the son-in-law's family, to the son-in-law himself, his father, brother or sister, and for his prospective children.

The transmission from individual to individual through marriage is most arbitrary. Thus we have one case in which a man (1) obtains his name and position (*a*) from his sister's husband (2), who had obtained it from his own father (3), who in his turn ob-

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tained it through a former marriage—not with the mother of the individual (2)—from his former wife's father. Diagrammatically this may be expressed as follows (fig. 5). In another case the father of a man was given a name and position by his daughter-in-law's father. In these cases the person who paid the marriage price to the bride's father receives the gifts returned by the father-in-law.

These names and positions, of course, cannot be actually taken until the son-in-law gives a feast at which the gifts are formally bestowed and at which the presents received from the father-in-law are distributed among the *numayms* of the son-in-law's tribe, excluding his own *numaym*. Practically the son-in-law is the recipient of these names, but they are given to him to be bestowed upon certain designated persons. In most cases the son-in-law who already holds a noble position, uses the new name and position that he himself received from his father-in-law only at the festival at which he distributes the marriage presents which he has received from his father-in-law, and then he "puts away" the name until he in turn gives it to his son-in-law or to some member of his son-in-law's family. There are, however, cases in which this is not done. Thus a noble chief of the Kwāg·uł gave up his position and took the place of his father-in-law who was a Mamalēleqala. The Kwāg·uł were dissatisfied with this arrangement and in order to adjust matters, he sent his second and third children to take his places in the *numaym* to which he belonged, while he himself, his wife, eldest and youngest sons took their places among the Mamalēleqala. Such a transfer of a son-in-law to his wife's *numaym* and tribe does not seem to be frequent, although it is permitted.

The actual position of the first born child is, therefore, that by birth it belongs to a certain *numaym* and that under normal conditions it will remain there and receive additional names and positions from its father-in-law. These, however, will be given up when his daughter marries and ordinarily descend to her son, although this is not absolutely necessary. Later born children are liable to attain high rank through marriage and will be more readily transfered through marriage to a new *numaym*. That the son-in-law

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has the free disposal of names given to him personally is brought out clearly by the fact that he can transmit them even after a divorce and a new marriage to the descendants of relatives of his new wife, or that he may bestow names received from his second wife to descendants or relatives of his former wife. It is also interesting to note that in some cases names and privileges received in marriage are split and become the property of different individuals.

The most common arrangement is that a man places his daughter's husband in one of the positions at his disposal, either his own or one belonging to him in some other way. The positions acquired by marriage are retransmitted in the same way, so that the holders will always be the husbands of a succession of daughters. The names and privileges are held by the men, although they descend throughout through the line of daughters. In the genealogies at my disposal I have not found cases of such continued transmission, neither do I find a continued transmission from maternal grandfather to grandson. There is rather a tendency for the lines transmitted through marriage to disappear. It is not safe, however, to infer from this that continued transmission through marriage does not occur, because the genealogies are naturally so arranged that the privileges of a certain noble person now living are accounted for. Owing to the fact that all the younger lines, in which privileges and position obtained by marriage are transmitted, have not been recorded and that the positions accounted for are generally in the line of primo-geniture, the disappearance of privileges obtained by marriage may be rather apparent than real. Transmission through the mother, *i.e.*, from the maternal grandfather to the grandson is found very frequently in the genealogies at my disposal, but it is not as frequent as direct transmission from father to son. In one genealogy, transmission from maternal grandfather to child appears fourteen times, from father to child, twenty-nine times.

Evidently the individual wish of a dying person regarding the disposition of his name, position, and privileges is one of the decisive elements in the assignment of social position. As long as any right can be construed that justifies the desired transfer, the *numaym*

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will abide by the wish of the deceased. If, however, selection can in no way be justified by the laws of descent, the *numaym* may not permit the proposed transfer.

In those cases in which the disgrace of illegitimate descent, *i.e.*, descent from a couple who did not go through all the formalities of a marriage, attaches to the proposed successor, he may not be admitted to the positions bequeathed to him. The effect of such a disgrace is illustrated by the following example. A man, who belonged to the *numaym* mentioned before, which is considered as descended from slaves who were not married according to the customary form, was considered as of lower rank because he belonged to this *numaym*. Furthermore, his parents were not properly married and he himself lived with a woman of high rank without performing the proper marriage ceremonies. He became very wealthy and inherited a number of high positions. The *numayms*, however, will not allow his children to take his place. His name is to die and the children will be assigned to positions in the mother's *numaym*. Although they will assume high positions, their descent will always be felt as a blemish. I presume in early times, when other individuals of pure descent were available, they would not have been permitted to occupy these positions.

The wish of the dying person may also be vetoed by a member of his family who has a nearer right to succession than the designated successor.

According to the ideas of the Indians, the two categories of names and privileges, those in the line of primo geniture and those that may be transferred by marriage, are quite distinct. Nevertheless, the law of preventing the transfer of the inalienable family names and privileges to another family is broken every now and then, in accordance with the wish of the holder of the place expressed on his death-bed. I do not doubt that in early times, when the claims of the individual could be maintained by force, a usurped position could be held, provided the holder had sufficient strength to withstand his rivals and enemies. The law may also be broken when a tribe or *numaym* demands that one of the descendants of a chief be made his successor.

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The names and privileges belonging to the line of primogeniture and those that may be transferred by marriage are of the same character, excepting only a number of offices like that of the Keeper of the Order of Seats in a *numaym*. There is no reason that would compel us to assume that the two sets have distinct origins. It may rather be assumed that certain privileges and names that have been transmitted in a family for a long time, were considered as the inalienable property of the family. There is an unsurmountable contradiction involved because the Indian theory requires that from the very beginning there must have been these two classes of privileges, a condition that does not seem tenable. If, however, we project modern conditions into the past and assume as an early custom, the arbitrary assignment of a child to one place or another according to the wish of the parents, and according to the right which the child holds by reason of his descent, then the present order is quite intelligible. We must assume that certain privileges were given away, while others which were considered more valuable were retained in the line of direct descent. In this manner, a division between the two groups of names and privileges may have developed. We may perhaps compare the conditions to the European Majorate and to the transmission of family heirlooms as against free disposal of other property. The law is not so rigid that we could speak of an entailment of certain names and privileges because it is sometimes broken. The transfer by marriage may be compared to those cases in which jewelry is handed down in a family to be worn by the eldest son's wife.

I do not see any reason for a change of my opinion in regard to the relative antiquity of the transfer of names and privileges through the male or the female line. It is, of course, impossible to obtain historical data that would prove the actual development and we can only discuss the probable course of events. I base my argument largely upon the general cultural assimilation between the Bellacoola and the Kwakiutl tribes and the Nootka on the one hand, and their northern neighbors on the other hand. Linguistically, the Bellacoola are closely associated with the Coast Salish. Vocabulary and structure prove that at one time the two groups

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must have been one and must have lived on the seacoast. All the Coast Salish tribes, with the exception of the Bellacoola, are organized in simple village communities with preponderent patrilineal descent. Village communities may still be recognized as the fundamental divisions of the Bellacoola, but the organization is overlaid by the use of crests and privileges which are characteristic of the Tsimshian and of the northern Kwakiutl. The forms and names of privileges and the names of individuals using the privileges prove the most intimate association with the neighboring tribes. Similar conditions, only less developed, may be traced among the northern Coast Salish, who have adopted privileges for a few social units while other social units have no such privileges. On Fraser river these ideas have even penetrated to the Lower Thompson tribes and northward to the Lillooet. The Kwakiutl are so thoroughly saturated with the use of privileges that no essential differences can be discovered in the various groups. Unfortunately, we do not know enough about the northern Kwakiutl tribes to state definitely the conditions prevailing among them. The observations among the southern tribes, however, make it clear that among the southern Kwakiutl, as well as among the Nootka and the Coast Salish, the village community is conceived as a closed group and forms the basis of modern social organization. The exogamous lines, which are superimposed upon the village communities and embrace all of them, and which are an essential feature of the social system of the northern tribes, do not occur. The fragmentary archaeological evidence which we possess from the Kwakiutl territory suggests that the whole elaborate artistic development of the crest is not very old. Even the remains from graves that belong approximately to the middle of the last century indicate that the complete crowding out of geometrical ornament by conventional animal representations occurred quite recently.

It appears to me largely as a psychological question how the highly specialized use of privileges may have been superimposed upon an older simple organization which has a rather wide distribution on the coast. There is nothing to indicate that the simpler form should have been developed from a totemic organization.

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The evidence appears to me rather the other way. If Dr. Farrand's and my own observations are correct, namely, that the prevailing line of descent among the northern Kwakiutl tribes is matrilineal, then it seems to me plausible to assume that in marriages between men of those tribes and Tsimshian or Haida women, privileges were imported which the foreign born women could transmit according to the customs of their own tribes only to their own children and through their daughters to their grandchildren, but not to the children of their sons. The conditions of life on the coast indicate that the possession of such privileges was felt as a great social advantage to which the owners would cling. Since the Kwakiutl do not permit transfer from a man to his sister's sons, it would seem natural that the characteristic method found in mythological tales of the acquisition of Tsimshian privileges and which is even nowadays practised in the potlatch, should be adopted. This method is the transfer of privileges by gift from the husband's family to the wife's family. When a northern woman marries a Kwakiutl man, her son would be entitled to her crests. Since according to the property rights of the Kwakiutl, he could not transmit them to his sister's children, the possibility presented itself, to transmit them as a present to the family of his daughter's husband and to secure in this way the transmission to her children. As stated before, the mythological data indicate that this custom must have prevailed among the northern tribes.[1] Perhaps I have myself unwittingly contributed to the disagreement of opinion in regard to the historical development of the social organization of this area. When I stated that in my opinion maternal descent was later than paternal, I did not point out specifically the difference between the type of maternal descent as usually conceived and that prevailing on the Northwest coast, because it seemed to me obvious that we have no trace of the characteristic succession from uncle to sister's son, but only a somewhat cumbersome transmission of privileges from daughter to daughter in which the husband is the bearer of name and privileges.

[1] Leonhard Adam has called attention to the discrepancy between tradition and modern customs to which I referred above. *Ztschr. f. vergl. Rechtswissenschaft*, vol. XXX (1913): p. 193; also Boas, "Tsimshian Mythology," *Thirty-first Annual Report of the Bureau of American Ethnology*, p. 528.

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The fundamental difference between the organization of the Kwakiutl and the northern tribes appears also in the terminology of relationship. Their terms are throughout the same for the paternal and maternal lines; uncle and aunt, nephew and niece are terms used indiscriminately for father's and mother's brothers and sisters and for brother's and sister's children without regard to the sex of the speaker. There is no trace of the recognition of clan or gentile relationship. The terms correspond to a loose organization in which relationship is counted equally on both sides.[1] The terminology by which individuals are called members of tribes indicates decidedly a preference to the father's side. The child of a father belonging to one tribe and of a mother belonging to another tribe is designated by the name of the father's tribe or *numaym* with the ending *-ts!ɛdze*, *i.e.*, offspring of such and such a tribe or *numaym*. The mother's tribe is indicated by her tribal name and the ending *-k'!otɛm*, *i.e.*, one side of face such and such a tribe. Furthermore, in a marriage between two members of different tribes, the wife is called "married far outside." This agrees with the custom that in by far the majority of cases the woman goes to live with her husband, as well when both belong to the same village as when they belong to different villages.

It appears from all that has been said that the privileges are individual property, not property of the whole *numaym*, so that the social divisions are not in any sense properly speaking totemic groups. The relation to a very generalized form of the clan crest which belongs to every member of the clan, which is characteristic of the northern tribes is absent here. Common to the northern tribes and to the Kwakiutl is only the personal privilege of persons of high rank to certain specific crests and privileges. Among the Kwakiutl a new-born child has no crest and no definite position until it is given to him by his parent or another relative and there is no association between all the members of the *numaym* and the "totem." It is true that the nobility believe that they are des-

[1] Franz Boas, "Tsimshian Mythology," *Thirty-first Annual Report, Bureau of American Ethnology*, p. 494. The term 'nɛmwōt which is given at that place as relating only to members of the family, is used very often as applying not only to the *numaym* but also to outsiders, friends.

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cended from an ancestor who had the form of an animal, a whale, killer-whale, or supernatural bird, "who took off his mask and became secular." As stated repeatedly, this does not refer to persons of low rank and not even to all *numayms*. Individually the belief may arise that a person is helped by his crest animal. Thus a Nāk!wax·da'x$^{u}$ chief of the *numaym* 'wālas sacrificed to the killer-whales and was believed to be helped by them, but this was felt by the Indians as something quite unusual. In regard to other animals the evidence is contradictory. I have been told that a *numaym* which has the bear for a crest will be helped by the bear, but others flatly contradict such an idea. The statement that the Thunder-bird the ancestor of the G·ig·îlgăm of the Nimkish, thunders whenever one of the *numaym* (probably one of the chiefs) dies may also be mentioned here.[1]

The essential feature of the relationship of the whole *numaym* to an animal is either entirely missing, or at least very weakly developed.

There is nothing to indicate that these forms are broken down remains of an older true totemic organization. The close relationship between Kwakiutl organization and that of the Coast Salish and the ideas clustering around the crests make it much more plausible that these semi-totemic notions may spring up every now and then without ever having been characteristic of the organization as a whole. I feel quite certain that the case of the relation of an individual to the killer-whale to which I referred just now, was developed by that particular person on the basis of the general beliefs of the tribe. Neither do I consider it a proof of older totemic ideas if a chief in a formal speech identifies himself with his animal ancestor who became "secular." This must be taken as no more than a metaphorical expression similar to those in which he calls himself the "Pillar of Heaven" or "Rockslide" or "River of Wealth." We must not interpret an oratorical metaphor as having a deeper religious significance, although it may stimulate thought in directions that may lead to religious tenets.

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[1] Franz Boas, "Kwakiutl Tales," *Columbia University Contributions to Anthropology*, vol. II, p. 85.

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# LAS EXCAVACIONES DEL PEDREGAL DE SAN ANGEL Y LA CULTURA ARCAICA DEL VALLE DE MEXICO

POR MANUEL GAMIO

EN 1884 el Profesor Holmes mencionó en uno de sus estudios varios ejemplares de cerámica del Valle de México que de acuerdo con su descripción[1] corresponden a la cerámica que actualmente se denomina de tipo arcaico.

En 1907 la Señora Zelia Nuttall sacó de debajo de la lava en las canteras de Coyoacán huesos humanos calcinados y cerámica, y empezó a coleccionar en otras regiones del Valle antigüedades pertenecientes al mismo tipo cultural, las cuales enseñó a varios arqueólogos.

Durante el año de 1909 hice un reconocimiento en las regiones que comprende la Municipalidad de Azcapotzalco, Distrito Federal, México, y pude identificar el carácter prehispánico de los vestigios que ahí se encuentran, los cuales consisten en depósitos de cerámica contenidos en los terrenos sedimentarios de la llanura y en montículos artificiales que ocultan en su interior restos de estructuras arquitectónicas. Entre los ejemplares extraídos de tales depósitos se contaban diversas representaciones de forma humana de barro y fragmentos de la cerámica posteriormente filiada como de tipo arcaico. Las colecciones obtenidas durante la exploración fueron entregadas al Museo Nacional, en cuyos Anales se publicó el estudio que hice sobre el particular.[2]

Posteriormente el Dr. Seler encontró cerámica del tipo arcaico en la misma región.

En el interesante "Manual de Arqueología" publicado por el Dr. Herbert J. Spinden,[3] se dice al comenzar el capítulo referente

[1] William H. Holmes, "Antiquity of Man on the Site of the City of Mexico," *Transactions of the Anthropological Society of Washington*, vol. III, 1885.

[2] Manuel Gamio, "Restos de la Cultura Tepaneca," *Anales del Museo Nacional de Arqueología, Historia y Etnología*, Nueva Serie, Tomo I. México, Sept. de 1909.

[3] Herbert J. Spinden, *Ancient Civilizations of Mexico and Central America*, New York, 1917.



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al "horizonte arcaico" que en 1910 fué encontrada en los alrededores de México una estratificación que presentaba tres tipos distintos, entre ellos el arcaico; pero tenemos entendido que entonces no se escribió estudio alguno sobre el particular.

En 1911 el Dr. Franz Boas, Director de la Escuela Internacional de Arqueología y Etnología Americanas, colectó algunos millares de fragmentos de cerámica del Valle.

El Dr. Boas clasificó esos ejemplares en tres grandes grupos de acuerdo con su procedencia y publicó sus reproducciones en el "Album de la Escuela Internacional"[1] siéndome encomendada la escritura del texto relativo, pues entonces era alumno de la citada Escuela. Por diversas circunstancias no se ha podido publicar ese texto; pero actualmente, que tengo el honor de ser Director de la Escuela Internacional, hago gestiones conducentes a su publicación.[2]

Corresponde al Dr. Boas la primera descripción del tipo arcaico, al que llamó "de los cerros," según puede demostrarse por los siguientes conceptos que estampó en las publicaciones de la Escuela:[3]

III. Tipo de los Cerros. En las faldas de muchos cerros del Valle de México se encuentran tipos de cerámica muy distinta a la de los Aztecas. No hay el tipo amarillo ni el rojo; en vez de ellos se hallan vasos muy gruesos, con decoraciones pintadas en rojo o grabadas. Los lugares en donde se ha estudiado este tipo son la Sierra de la Estrella, los Reyes, el Peñón de los Baños, Zacatenco y Ticumán. Las orillas de muchos vasos tienen decoraciones plásticas. A las asas se les da la forma de manos; las patas son muy gruesas. Un color blanco fijo se usa, distinto del blanco azteca que se borra fácilmente. Los dibujos grabados consisten en curvas o en grupos de puntos, o en combinaciones de los dos, o en líneas angulares. Hay muchas cabecitas de un tipo muy distinto del tipo del Valle de México y del de Teotihuacán. La técnica es ruda; los ojos se indican por bolitas de barro que tienen dos impresiones, o por dos impresiones hechas en el barro con un palito. Otros detalles de forma también se hacen por barritas y bolitas de barro, no con moldes. Los tipos de vasos y de cabecitas son algo semejantes a los tipos de Michoacán. Mientras que los objetos recogidos en

[1] Album de Colecciones Arqueólogicas, Láminas 1–69. Seleccionadas y arregladas por Franz Boas. Ilustraciones por Adolfo Best. *Publicaciones de la Escuela Internacional de Arqueología y Etnología Americanas*, México, 1911–1912.

[2] Las colecciones de cerámica que se describen en dicho texto y que fueron reproducidas en el Album, existen en los salones de exhibición de la Dirección de Antropología.

[3] Escuela Internacional de Arqueología y Etnología Americanas. Año Escolar de 1911 a 1912. Exposición de Trabajos en la Sala de Conferencias del Museo Nacional de Arqueología, Historia y Etnología, del 8 al 15 de Abril de 1912. México. (Pág. 12.)

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la sierra de Guadalupe, en Zacatenco y en Ticumán tienen el mismo tipo, el de Culhuacán es distinto. Los dibujos grabados y las formas de los vasos tienen un carácter especial y tal vez en esta cultura hubo un desarrollo de tipos locales muy distintos de los que se encuentran en el período azteca.—En todos los sitios hay ruinas aztecas próximas y siempre se encuentra el tipo del cerro en la parte superior, cuyo declive se confunde con la llanura, mientras que el tipo azteca se encuentra en montículos, en el llano.

III. Antigüedad del Tipo de los Cerros. Los objetos del tipo de los cerros se encuentran en la superficie, o en taludes deslavados por las lluvias. Por eso al pie de los cerros están revueltos con restos aztecas, o forman capas que cubren a éstos. Sin embargo, a poca distancia de los cerros no se encuentran más que uno que otro objeto de esta clase en las capas superficiales, objetos que quizá se han llevado ahí por los habitantes de las poblaciones aztecas. Un número bastante grande se encuentra en las capas hondas de Culhuacán. Allí no se pudo continuar la investigación, porque el nivel del agua se encuentra antes de llegar al término de la cultura azteca.—Ya se habían encontrado objetos de este tipo en Clavería, cerca de Tacuba, por el señor Gamio; y en Azcapotzalco por el Sr. G. Niven y el Profesor Seler. También se habían encontrado allí muchos objetos del tipo de Teotihuacán y por esa razón parecía probable que la sucesión de civilizaciones se pudiera averiguar con éxito.

Aunque, como arriba dije, el tipo de los cerros había quedado inicialmente identificado por el Dr. Boas, este señor me encomendó la investigación de su antigüedad con respecto a la de los otros dos, azteca y teotihuacano, que aparecen en el Valle. Cumpliendo tal recomendación efectué la excavación de San Miguel Amantla, Azcapotzalco, y pude comprobar que en ese lugar se encontraban tres estratificaciones geológico-culturales superpuestas por orden de antigüedad, siendo la primera o superficial correspondiente al tipo azteca, la segunda al teotihuacano y la tercera o más profunda al "arcaico," que entonces denominé "de los cerros," siguiendo al Dr. Boas. Publiqué los resultados de estos trabajos con el título "La Sucesión de Tipos Culturales en Azcapotzalco" en las mismas publicaciones en que el mencionado señor identificó el tipo de los cerros.[1]

Contando con mejores datos publiqué sobre el mismo asunto el artículo "Arqueología de Azcapotzalco" en las publicaciones del XVII Congreso Internacional de Americanistas.[2]

[1] Manuel Gamio, "La Sucesión de Tipos Culturales en Azcapotzalco," *Escuela Internacional* (opúsculo citado, pág. 13).

[2] Manuel Gamio, "Arqueología de Azcapotzalco, D. F., México," *Proceedings of the Eighteenth International Congress of Americanists.* London (pág. 180).

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La excavación hecha en San Miguel Amantla, Azcapotzalco, fué considerada por mí como tipo metodológico, pero no como tipo de sucesión cultural regional, ya que sólo en un lugar se halló la sucesión estratigráfico-cultural en el orden que antes se indicó. Debe advertirse que hasta esa fecha dicha excavación fué la primera y única que se efectuaba con método científico en el Valle de México; así que no podían obtenerse otras conclusiones que las arriba indicadas. Sin embargo, posteriormente se aventuraron exageradas conclusiones basándose exclusivamente en los resultados obtenidos en la excavación de San Miguel Amantla, por más que no se citó dicha excavación.

Durante el año escolar de 1912–13 el suscrito, que todavía era alumno de la Escuela Internacional dirigida por el Sr. Prof. D. Jorge Engerrand, efectuó en la misma región de Azcapotzalco cuatro excavaciones, siento tres de ellas en el atrio de la Parroquia y la otra en los campos adyacentes al poblado de Santa Lucía. Los resultados de tales excavaciones fueron expuestos en las publicaciones de la Escuela[1] y más tarde en las publicaciones del XIX Congreso Internacional de Americanistas[2] y consistieron sintéticamente en que en los lugares explorados no apareció representada la cultura arcaica y en cambio la azteca y la teotihuacana sí aparecieron, si bien confundidas en todos los estratos.

En cuanto a la extensión geográfica de la cultura arcaica en América, se han emitido diversas opiniones que desgraciadamente no ofrecen convencimiento satisfactorio. Sin embargo, pueden hallarse interesantes observaciones sobre el particular en el Manual del Dr. Spinden, a que antes hicimos referencia.

De las investigaciones de que se ha hecho mención en este capítulo de antecedentes puede deducirse en resumen lo siguiente:

La cultura o civilización arcaica, la teotihuacana y la azteca son las únicas que florecieron en el Valle de México.

Aunque en la excavación estratigráfica de San Miguel Amantla,

[1] Jorge Engerrand, Director, *Discurso Inaugural y Reseña de la Exposición Anual de la Escuela Internacional de Arqueología y Etnología Americanas.* México, 1913.

[2] Manuel Gamio, Investigaciones Arqueológicas en México, D. F., 1914–15, *Proceedings of the Nineteenth International Congress of Americanists*, Washington, December, 1915–17, pág. 131.

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la cultura arcaica es la más antigua pues aparece en los estratos más profundos, no puede generalizarse razonablemente por este solo caso, atribuyendo a dicha cultura la misma antigüedad en toda la extensión del Valle.

Más adelante se expondrán las conclusiones obtenidas como resultado de las excavaciones hechas en el Pedregal de San Angel, bajo la dirección del suscrito y vigilancia inmediata de los Sres. Ing. José Reygadas Vértiz y Gabriel Gamio. Sobre estas excavaciones se han emitido ya algunas opiniones.[1, 2, 3, 4, 5]

## Aspecto Geologico Regional

En épocas remotas se extendía desde lo que es hoy la población de San Angel hasta las de Coyoacán y Tlalpan, una llanura de suave inclinación, irrigada por las corrientes que descendían de la serranía del Ajusco. Los hallazgos e investigaciones a que nos referiremos más adelante permiten afirmar que esa llanura fué habitada por una numerosa población. Posteriormente, erupciones de cráteres de la citada serranía, arrojaron cenizas y lavas hasta cubrir la llanura con una extensa capa de roca volcánica cuyo espesor varía de 4 a 8 metros, y que es conocida con el nombre de "Pedregal de San Angel." En este enorme depósito de lava existen las canteras que han suministrado piedra para la construcción de casi todos los edificios de la Capital de la República.

Desde hace bastante tiempo se habían encontrado, al explotar dichas canteras, fragmentos de cerámica y huesos humanos y de animales, de los que hicieron mención algunos investigadores,

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[1] Hermann Beyer, S. N. A. México, "Sobre Antigüedades del Pedregal de San Angel," *Memorias de la Sociedad Científica "Antonio Alzate,"* Tomo 36, Núm. 1. México, Octubre de 1917.

[2] Alfonso Toro, "El Hombre del Pedregal de San Angel," *Revista de Revistas.* México, Núms. 419, 420, 421, 422, Mayo y Junio de 1918.

[3] "Los Misterios del Pedregal de San Angel" (Sensacional Descubrimiento Científico Mexicano), *Revista de Revistas*, 1918, Enero 6 y 13, Núms. 401 y 402.

[4] Lic. Ramón Mena, "El Hombre del Pedregal" (Conferencia), Escuela Nacional Preparatoria. Curso de 1918.

[5] Ing. José Luis Osorio Mondragón, "Los Descubrimientos del Pedregal," *Revista Mariana.* México 8 de Julio de 1918.

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quienes han publicado estudios teóricos sobre tan interesante asunto. [1, 2, 3, 4, 5]

Hasta 1917 se efectuaron excavaciones metódicas que pueden ofrecer datos experimentales de valor positivo.

En efecto, en el mes de agosto de 1917, el personal de la Dirección de Estudios Arqueológicos y Etnográficos—hoy Dirección de Antropología—procedió a hacer un reconocimiento métodico de todas las canteras que estaban en explotación, hasta conocer en cuál de ellas era más frecuente la presencia de cerámica fragmentada y otros vestigios, resultando que en el lugar donde se notaba más abundancia, era en la Cantera de Copilco, colindante con la Colonia del Carmen en la población de San Angel.

Desde el punto de vista geológico pueden distinguirse en las canteras de Copilco tres capas claramente diferenciadas: (fig. 6)

*A*. Capa de lava volcánica.

*B*. Terreno de estructura blanda en el que aparecen los vestigios arqueológicos y restos humanos.

*C*. Terreno de estructura compacta en el que fueron excavados los sepulcros cilíndricos.

En cuanto a la formación geológica y a la composición mineralógica de esos terrenos, pueden adquirirse amplias y autorizadas opiniones en estudios especiales hechos sobre el particular.[6, 7, 8]

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[1] Mariano Bárcenas, "Nuevos datos acerca de la antigüedad del hombre del Valle de México," *La Naturaleza*. Tomo 7, págs. 265–266 y 270. México, 1885–86

[2] Alfonso L. Herrera, "El Hombre Prehistórico de México," *Memorias de la Sociedad Científica "Antonio Alzate,"* Tomo 7 (1893).

[3] Mariano Bárcenas, "El Hombre Prehistórico de México," *Actas del XI Congreso Internacional de Americanistas*. México, 1895–97.

[4] Manuel M. Villada, "El Hombre Prehistórico del Valle de México," *Anales del Museo Nacional de México*, Tomo VII (1903).

[5] Francisco Fernández del Castillo *Apuntes para la Historia de San Angel*, (San Jacinto Tenanitla), y sus alrededores. México, 1913.

[6] Ezequiel Ordóñez, "El Pedregal de San Angel," *Memorias de la Sociedad Científica "Antonio Alzate,"* Tomo VI (1893).

[7] W. Wittich y P. Waitz, "Tubos de Explosión en el Pedregal de San Angel," *Boletín de la Sociedad Geológica Mexicana*, Tomo VII (1911), 2ª parte.

[8] Dr. E. Wittich, "Fenómenos microvolcánicos en el Pedregal de San Angel," *Memorias de la Sociedad Científica "Antonio Alzate,"* Tomo XXXVIII (1919), Núm. 3.

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FIG. 6.—Vista General de las Canteras de Copilco. San Angel, D. F.

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## Aspecto Arqueologico de los Vestigios

En tres grupos pueden dividirse los vestigios arqueológicos descubiertos en los cuatro túneles (T.[1] T.[2] T.[3] T.[4],) (fig. 7) que fueron excavados en la Cantera de Copilco:

1º. Sepulcros.

2º. Pavimentos e hileras de piedras.

3º. Objetos de barro y piedra.

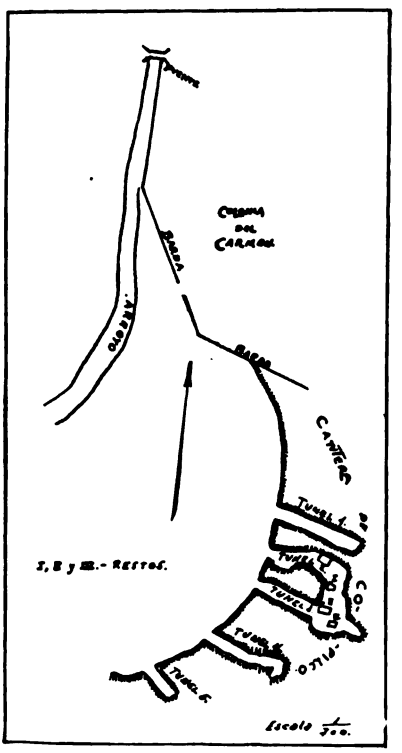



Fig. 7.—Tuneles Practicados en la Cantera de Copilco. San Angel, D. F.

### *Sepulcros*

Consisten en oquedades cilíndricas excavadas en el tepetate o terreno compacto sedimentario (*C*). Fué encontrado un sepulcro en cuyo fondo pueden distinguirse los huesos de un miembro inferior en flexión. Junto a las osamentas aparecieron vasijas y objetos de piedra que más adelante se mencionarán. No todas las osamentas estaban enterradas en sepulcros cilíndricos, pues las que existen en el Túnel T.[2] fueron depositadas en la capa de terreno sedimentario blando (*B*).

En el Tunel T.[4] se encontraron huesos contenidos dentro de una vasija. Encima de los sepulcros cilíndricos había montículos formados por grandes cantos rodados.

### *Pavimentos e Hileras de Piedras*

Una característica muy interesante y que no ha podido ser satisfactoriamente explicada consiste en una serie de líneas o hileras de piedras o cantos rodados, que estaban colocadas en la superficie

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del terreno antes de la erupción, como lo demuestra el hecho de que actualmente están dichas piedras inmediatamente abajo de la lava.

Asimismo existen en algunos lugares cantos rodados dispuestos con regularidad, como si hubiesen formado pavimentos.

### *Objetos de Barro y Piedra*

Los objetos de barro encontrados se dividen en esculturas antropomórficas y en vasijas. La figura 8 representa las primeras,



FIG. 8.—Esculturas Antropomorficas. Cantera de Copilco. San Angel, D. F.

y las figuras 9 y 10 representan algunas vasijas, así como sus cortes y decoraciones.

La índole de este artículo nos impide entrar en pormenores de clasificación en esta cerámica, que será ampliamente descrita en el estudio que sobre las excavaciones del Pedregal de San Angel prepara esta Dirección. Por otra parte, en el texto del álbum de la Escuela Internacional a que antes se aludió, se describe gran número de cabecitas y cuerpos de barro y fragmentos de cerámica arcaica. Sí creemos indispensable hacer notar que cerca de todas las osamen-

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tas aparecen vasijas probablemente rituales y en algunos casos pequeñas figurillas de barro de forma humana y fragmentos de vasijas policromamente decoradas.

Los objetos de piedra consisten principalmente en metates o morteros; en piedras que tienen la forma de prismas triangulares y en masas lenticulares para moler cereales en dichos morteros; dardos y flechas de obsidiana; esferas de diversos tamaños; pequeños bloques agujereados que sugieren malacates primitivos, etc.

## Aspecto Anatomico de las Osamentas

En los sepulcros de que se habla en el "Aspecto Arqueológico" se encontraron varios esqueletos de adultos y niños, cuya consistencia deleznable fué causa de que unos aparezcan incompletos y otros en estado fragmentario.



FIG. 9.—Vasijas. Cantera de Copilco. San Angel, D. F.

Para examinar estas osamentas se comisionó a los estudiantes de Antropología de esta Dirección, Paul Siliceo Pauer, Abel Díaz Covarrubias y Bernardo Reina, quienes estudiaron de preferencia la osamenta más completa y mejor conservada que existe en el Túnel T.[3] De dicho estudio se hizo el resumen siguiente:

"Los restos pertenecen a un adulto como de treinta años de edad, de una estatura de 165cs., del sexo masculino y de constitución robusta.

Están orientados de E. a W. y descansan en decúbito lateral derecho. El brazo del mismo lado un poco hacia atrás del cuerpo sobre su cara externa y la mano sobre su cara palmar. El brazo izquierdo se encuentra a lo largo del tronco quedando su extremidad inferior un poco adelante de la línea axilar anterior; el antebrazo del propio lado cruza diagonalmente al abdomen, vuelta hacia

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éste último su cara anterior y formando con el brazo un ángulo poco mayor que el recto; la mano por su cara palmar toca el borde anterior del hueso ilíaco derecho. La pelvis y los miembros inferiores reposan sobre su cara anterior, excepto la pierna izquierda, que se apoya por su cara interna.

La cabeza, deformada por la presión de la lava, descansa sobre el lado derecho. Tiene la forma de un ovoide cuya extremidad mayor está dirigida hacia atrás y abajo; su extremidad menor o anterior, bastante oblicua, presenta el tipo perfecto de frente fugitiva. Su diámetro antero-posterior es de 174mm., el diámetro antero-posterior iniaco de 171mm. y de 116 el transverso máximo. Este último y el primero están en la proporción de 1 a 1.5.



FIG. 10.—Cortes y Decorationes de Vasijas. Cantera de Copilco. San Angel, D. F.

El diámetro transverso, medido indirectamente, lo encontramos igual a 116mm.; admitiendo un error de 10mm. en más o en menos, obtendríamos un índice craneano de 60.9, en caso de ser igual a 106mm., si fuere de 116mm., un índice igual a 66.6 y por último en caso de ser igual a 126mm., un índice de 72.4, quedando en los tres casos comprendido en el grupo de los dolicocéfalos.

Como el diámetro basilo-bregmático es igual a 135mm., la altura del cráneo es un poco mayor que los dos tercios de su longitud, dando un índice vertical de 77.5, que corresponde al de un individuo de tipo hipsicéfalo.

El diámetro naso-basilar mide 99mm. El bi-mastoideo, tomado también indirectamente, es de 104mm.

La curva antero-posterior, tomada sobre la línea media, alcanza un desarrollo de 357mm., repartidos como sigue:

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| | |
|---|---|
| Del nasio al bregma | 123mm. |
| Del bregma al lambda | 127mm. |
| Del lambda al inion | 64mm. |
| Del inion al opistio | 43mm. |
| Total | 357mm. |

La curva horizontal es de 472mm.

El agujero occipital tiene una longitud de 40mm. por una anchura de 38mm., no siendo posible dar su grado de inclinación.

Lo reducido de la glabela y el escaso desarrollo de los arcos superciliares inducen a suponer que son muy pequeños los senos frontales. La cresta lateral del frontal y las líneas semicirculares del parietal, circunscriben una fosa temporal de dimensiones poco mayores que las normales. No se encuentran huellas de la sutura metópica, ni de los agujeros correspondientes a la vena emisaria de Santorini y condileano posterior. Las suturas corresponden al núm. 5 del cuadro de complexidad de las mismas en la escala de Broca y al núm. 4 del grado de soldadura. En la sutura lambdoidea se encuentran dos huesos wornianos de escasas proporciones.

La apófisis marginal se ha desarrollado notablemente presentándose bajo la forma de una laminilla triangular de 15mm. de altura por 20mm. de base; este crecimiento es anormal.

La capacidad craneana obtenida de 1300cc. es solamente aproximada en virtud de la deformación que presenta el cráneo.

La cara no es visible en toda su extensión por estar destruido el macizo que forma el ángulo supero-externo de la órbita izquierda y oculto parte del maxilar inferior por una vasija de barro.

Los huesos propios de la nariz presentan una altura de 23mm. por 8mm. de anchura máxima.

La forma de la abertura de las fosas nasales difiere de la que tienen los tipos descritos por Topinard, denominados tipo europeo y tipo de bordes redondeados. En el presente caso es piriforme y la continuación entre el piso de las fosas nasales y el primitivo hueso inter-maxilar forma las canaladuras simianas.

El diámetro tranverso máximo de las fosas nasales es de 25mm.; la altura tomada del piso de ellas al nasio, es de 51mm. Su índice nasal de 49.0 corresponde al grupo de los mesorrinios.

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La espina nasal se encuentra muy reducida y el prognatismo del borde alveolar es considerable, aunque no puede consignarse su grado por no haber sido posible tomarlo.

De las órbitas, la izquierda se halla destruida y la derecha deformada por la desarticulación de las apófisis orbitaria externa y marginal, lo que ha motivado un alargamiento de su diámetro transverso que la ha hecho adquirir una forma ovalada cuyo diámetro mayor es de 43mm.; tiene una altura de 36mm.

Si las apófisis marginal y orbitaria externa volvieran a su sitios, la anchura orbitaria disminuiría coincidiendo aproximadamente con la altura y se obtendría un índice cercano a 100mm., mientras que en la posición en que se encuentra es de 119mm.

La anchura inter-orbitaria de dacrion a dacrion es de 17mm.

Del maxilar inferior solamente es visible la rama ascendente; el cóndilo tiene una dirección casi transversal. Los surcos y rugosidades de la cara externa de esta rama muestran la extraordinaria potencia de los músculos masticadores, y mide:

| | |
|---|---|
| Longitud | 70mm. |
| Anchura máx | 44mm. |
| Anchura mín | 37mm. |

La columna vertebral tiene una longitud de sesenta centímetros correspondiendo a la:

| | |
|---|---|
| Porción cervical | 15cms. |
| " dorsal | 38.5cms. |
| " lombar | 6.5cms. |

El diámetro transverso a nivel de la 4a. vértebra cervical es de 52mm.

Además de las curvaturas que le son propias, presenta otra que va desde la 7a. dorsal hasta la 5a. lombar, de concavidad vuelta a la izquierda.

En el sacro, visible por su cara posterior, el tubérculo correspondiente a la 2a. véretbra sacra está convertido en una verdadera apófisis espinosa.

No se logró encontrar el coxis.

Las costillas conservan su curvatura normal en el lado izquierdo, en tanto que las del derecho se hallan aplastadas, dirigiéndose directamente de arriba hacia abajo.

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El homoplato está casi destruido, conservándose únicamente la fosa supra-espinosa, parte de la espina, pequeños fragmentos de los bordes superior y axilar, así como las apófisis coracoides y acromial.

La clavícula izquierda es solamente visible en su extremidad externa; la derecha se halla destruida en su porción terminal.

El húmero tiene una longitud de 292mm. Se ven, aunque incompletas, sus tres caras. En la extremidad superior se ve el borde posterior del troquin encontrándose destruido el resto. La cabeza del húmero izquierdo no es visible y en el derecho solamente lo es en parte.

De la extremidad inferior se ve la cara posterior que tiene una anchura de 57mm. La foseta olecraniana es profunda, no comunica con la coronoides. Tiene una altura de 29mm., una anchura de 27mm. y una profundidad de 12mm.

La epitroclea se nota claramente; presenta rugosidades muy marcadas; el epicóndilo está destruido.

El cúbito del lado derecho descansa sobre su cara posterior, siendo visible solamente parte de la diáfisis; el del lado izquierdo descansa sobre su cara interna y se encuentra muy destruido.

Del radio es visible la mitad superior notándose la tuberosidad bicipital bien desarrollada.

La extremidad inferior está destruida totalmente.

No pudieron conservarse los huesos que constituyen el esqueleto de la mano.

Los dos huesos ilíacos fueron descubiertos en su mayor parte y se pudo ver la cara externa de ellos; se encuentran bastante fragmentados, por lo que sólo fueron tomadas algunas medidas.

Diámetro transverso máximo de una a otra espina iliaca anterior y superior, 25cms. De la escotadura ciática a la espina anterior y superior, 91mm. La altura del hueso ilíaco de la tuberosidad izquiática a la cresta ilíaca, 208mm. La altura de la cavidad cotiloides mide 54mm.

El fémur tiene una longitud de 423mm. Descansan ambos sobre su cara anterior y se encuentran destruidos en parte los cóndilos así como el gran trocanter; de la cara anterior a la línea áspera, en la parte media del cuerpo, mide 23mm.

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El cuello tiene una longitud de 44mm. por una anchura de 33mm.; forma con el eje del hueso un ángulo aproximado de 152.°

La tibia mide 385mm. de longitud por una anchura de 32mm. La derecha descansa sobre su cara antero-posterior y la izquierda sobre la cresta.

Nótase en la extremidad superior de la tibia derecha la parte interna del platillo.

Las extremidades de los huesos peronés no existen, por lo que no fueron medidos.

Los huesos del tarso están destruidos casi en su totalidad pudiéndose notar solamente una pequeña porción del calcáneo derecho. No existen ninguno de los huesos del metatarso, ni de las falanges."

## Conclusiones

Los descubrimientos hechos debajo del Pedregal de San Angel, en la Cantera de Copilco, así como el examen de los objetos procedentes de las demás canteras del mismo Pedregal que el personal de esta Dirección ha colectado, permiten emitir las siguientes sucintas conclusiones relativas al carácter de la civilización, al tipo físico, a la determinación cronológica y a la clasificación histórica de la población que habitó los lugares posteriormente cubiertos de lava volcánica y que son conocidos actualmente con el nombre citado de Pedregal de San Angel.

### *Carácter de la civilización*

La civilización de esos pobladores es la que se denominó en primer término "de los cerros" por el Dr. Franz Boas, después de "montaña" por el autor de estas líneas y por último "arcaico," por diversos americanistas como Spinden, Tozzer, Nuttall, etc., etc. Se justifica tal afirmación por la identidad que existe entre la cerámica arcaica hallada en diversos lugares del Valle de México y la que se encontró en el Pedregal.

La cultura arcaica había quedado exclusivamente caracterizada por su cerámica, pero los descubrimientos del Pedregal han aumentado el conocimiento de ella con varios objetos de piedra y de hueso que se describirán detalladamente en el estudio que está haciéndose

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en la Dirección de Antropología. Es digno de mencionarse el hecho de haberse encontrado también vestigios, ya sean rudimentarios, de construcciones, según lo demuestran los sepulcros cilíndricos cubiertos con montículos de piedras andesíticas rodadas y los pavimentos hechos con las mismas.

*El Tipo Físico de los Pobladores*

Estas exploraciones han permitido conocer por primera vez las osamentas de los hombres de civilización arcaica. Por el examen de las medidas expuestas se deduce que esos hombres son modernos, puesto que si bien presentan algunas anormalidades individuales, no se nota en ellos variaciones raciales sensibles, con respecto al hombre actual.

Confirmando esta opinión trascribimos lo que sobre el particular opina el Sr. Dr. Wittich en su publicación citada: ". . . el Pedregal de San Angel es una formación bastante moderna que representa el último paroxismo volcánico en el Valle de México, pero cuya edad absoluta no se puede precisar."

*Determinación cronológica*

Los objetos procedentes de todas las canteras del Pedregal son del tipo arcaico, no habiéndose encontrado uno solo de otro tipo cultural, ni siquiera de los tipos azteca y teotihuacano, los cuales en algunos casos han aparecido concurriendo con los de tipo arcaico en algunos otros lugares del Valle de México. Esto permite asegurar que la cultura arcaica del Pedregal es la más antigua del Valle de México y quizá de la República, pues sería imposible que si hubiese coexistido con otras culturas no se encontraran vestigios algunos de estas últimas, mezclados con las de aquélla. En efecto, en las numerosas excavaciones que hemos efectuado en diversas regiones de la República, no hemos encontrado un solo lugar, de igual o menor extensión que el Pedregal, donde existan perfectamente aislados los vestigios de una sola cultura como sucede en esta extensa región.

*Clasificación histórica*

En distintas ocasiones hemos dicho que las numerosas civilizaciones que la historia menciona con relación al Valle de México.

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deben ser referidas de acuerdo con lo que la arqueología ha demostrado de modo concluyente, a tres que son: la arcaica, la teotihuacana, y la azteca.

Es más o menos fácil investigar las denominaciones históricas que corresponden a las civilizaciones arqueológicamente caracterizadas como teotihuacana y azteca, pero había sido hasta hoy imposible hacer lo mismo con respecto a la civilización arcaica.

Felizmente, ya puede asegurarse de modo positivo, según quedó demostrado arriba, que la civilización arcaica es la más antigua del Valle, y de acuerdo con las fuentes históricas, la civilización arcaica identificada por la arqueología, es la civilización otomí a que se refiere la historia.

Para terminar, nos permitimos hacer una atenta proposición a los americanistas que dediquen en lo sucesivo su atención al interesante problema que entraña el conocimiento de la cultura arcaica: Por lo expuesto en las líneas anteriores puede notarse que reina alguna confusión con respecto a las denominaciones que ha recibido y sigue recibiendo la cultura "arcaica," de "cerro" o de "montaña." Sugerimos que siendo debajo de la lava del Pedregal de San Angel el primer sitio, y hasta hoy el único, en donde se han encontrado vestigios de la citada cultura, enteramente aislados e independientes de los de otras culturas, se denomine en adelante a la cultura discutida, "Cultura Sub-Pedregalense," denominación distintiva y justificada. Además, creo que, dada su antigüedad, deben ser considerados los vestigios del Pedregal como términos de comparación para el estudio y la clasificación de vestigios del mismo tipo que se encuentren en otras regiones cuya antigüedad no puede ser establecida con la certidumbre con que se ha conseguido hacerlo con los del Pedregal.

DIRECCIÓN DE ANTROPOLOGÍA,
MÉXICO, MARZO 10, DE 1919.



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# THE CULTURE PROBLEM IN OHIO ARCHAEOLOGY

By H. C. SHETRONE

THE archaeological area corresponding to the State of Ohio[1] is of particular interest and importance, from the fact that within its confines the great mound-building cultures of the Ohio and upper Mississippi valleys reached their highest development. At the same time, it presents a problem in prehistoric cultural distinctions and affinities which, owing to the complexity of its archaeology and the lack of ethnologic and historic evidence, is unusually difficult of solution.

The ideal anthropological area, in its simplest form, might be defined as one in which a surviving element of its prehistoric population can be definitely identified, through its ethnology and history, with the archaeological remains for which it is responsible. The Iroquois territory of New York state may be cited as approaching such an ideal area, since those groups of the Six Nations representing the prehistoric Iroquoian inhabitants are, in this manner, directly coordinated with the habitation sites of their early occupancy. Furthermore, the related Erie, while early disappearing from the area, left behind them historical evidence sufficient to identify their archaeological remains; and, finally, as a result of elimination, the prehistoric evidences of more remote Algonquian occupancy of the same region are differentiated from the preceding groups.

[1] The designations "Ohio archaeological area" and "native Ohio tribes" are here employed with a full realization of their inadequacy to express the exact meaning intended. The first-named term, used merely as a matter of convenience, seems justified by the fact that the two more important culture groups of the suggested area lie almost wholly within the state, although in reality such an area would embrace parts of those states immediately adjacent to Ohio.

With reference to a native Ohio tribe, it is obvious that the more or less nomadic habits of the American aborigine would tend to make his residence within a given locality of rather uncertain duration. In this paper the term is intended to represent those tribes who were resident or present within the area prior to historic record, and who were responsible for the archaeological remains thereof.

Use of the abstract noun, culture, to designate a specific social group, will be understood as a convenient abbreviation of culture group, or culture variety.



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Few areas, however, are so richly supplied with anthropological data as the example cited. In the eastern and southeastern districts, where white settlement was earliest, the anthropologist finds the usual archaeological evidence and rather abundant historic record, but a dearth of material for ethnological study. Proceeding westward, with the archaeological factor about constant, the ethnological element is augmented in approximately the same ratio that the historical is diminished.

The Ohio area, unfortunately, presents something of an anomaly, since ethnological data, in so far as its native tribes are concerned, are entirely lacking, while historic record, at first thought, would seem hardly more promising. Maps depicting the early linguistic areas of the United States[1] attribute the northern and northeastern portions of the state to the Iroquoian linguistic family, and the remaining one third to the Algonquian; but when reference is had to the distribution of this territory among specific Indian tribes,[2] we are confronted by blank space— the great Ohio country appearing as a no-man's-land, without hint of the lively human drama enacted on its soil in pre-Columbian times. It is hardly necessary to recall the fact that none of the several historic Ohio tribes was native to the state. All of them—the Miami, Shawnee, Wyandot, Delaware, Ottawa, Seneca, Mingo and others—had entered the territory within historic times, as a result, directly or indirectly, of unsettlement attending European colonization. Apparently none of them arrived sufficiently early to leave a very definite impression on the archaeology of the state.

The story of these exotic tribes during the years of their sojourn in Ohio is aside from a discussion of the native prehistoric tribes; but intervening between the probable time of disappearance of the latter and arrival of the former, there is a gap of upward of a century's duration, completely breaking the sequence of aboriginal occupation of the state, which properly may be considered in connection with the prehistoric period.

[1] Powell: Map of Linguistic Families; insert, vol. I.

[2] Wissler: Map of North American Indian Tribes; fig. 103.

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## The Period of Depopulation

The fact that the Ohio area, even for comparatively so short a time, should have been denuded of its native inhabitants, is unfortunate, to say the least, from an anthropological point of view. Occurring as it did just at the time when Eurpean exploration was pending but not yet accomplished, it severed the connecting chain between prehistoric and historic man in Ohio—a loss which only years of painstaking and persevering archaeological research, supplemented by the scant historic evidence, may hope to retrieve.

While for the present the passing of some of the groups composing Ohio's prehistoric population can be explained only in a general way—as the result of natural causes recognized by the anthropologist and the historian—it seems probable that the answer to the query as to the ultimate disappearance of the native tribes is to be found, in part, in certain historic records. Moreover, further archaeological evidence, of an important character, is to be had in the same quarter.

The so-called Iroquoian Conquest, culminating about the middle of the seventeenth century in the spectacular campaign which carried the supremacy of the Iroquois Confederation westward almost to the Mississippi river, has been accorded the prominence which it deserves as an event of general Indian history. Its importance, however, as a contribution to Ohio aboriginal history appears not to have been fully appreciated. Sufficient of the literature on the subject for the purpose in hand is concisely summarized in the following quotation:

> A factor which contributed greatly to the decline of the Algonquian ascendancy was the power of the Iroquoian confederacy, which by the beginning of the seventeenth century had developed a power destined to make them the scourge of the other Indian population from the Atlantic to the Mississippi and from Ottawa river in Canada to the Tennessee. After destroying the Huron and the Erie, they turned their power chiefly against the Algonquian tribes, and erelong Ohio and Indiana were nearly deserted, only a few villages of Miami remaining here and there, in the northern portion. The region south and west they made a desert, clearing of native inhabitants the whole country within five hundred miles of their seats. The Algonquian tribes fled before them to the region of the upper lakes and the banks of the Mississippi, and only when the French had guaranteed

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them protection against their deadly foes did they venture to turn back toward the east.[1]

If the writer may presume to interpret the Iroquoian Conquest in its relation to Ohio aboriginal occupancy, his conclusions are as follows:

The historic record of the event affords the only specific record of a native Ohio tribe—the Eries, or Cat nation—and directs attention to their archaeological remains in the territory which they occupied.

It offers an explanation of the fact that the greater part of the Ohio area remained for practically a century an uninhabited country—and incidentally, perhaps, throws some light on the cause of the early disappearance of the mound-building trait in the territory.

It suggests the authorship of a group of Ohio archaeological sites and artifacts hitherto unidentified, and which the writer ventures to designate as Algonquian.

The first-named of these three items may be considered as the only direct historic record of importance bearing upon the native or prehistoric tribes of the state, and with its consideration we may pass from the realm of history to that of archaeology. The line of demarcation, even here, is not sharply defined; for while the evidence itself is historic, the Erie nation, with which it has to do, might almost as well be considered as prehistoric, since so little actually is known of them. While the literature pertaining to the Erie is not entirely confined to the narration of the Iroquoian Conquest, that event, in its larger aspect, and as recorded in the Jesuit Relations,[2] may be considered as the original source of information.

From the Relations it is learned that the Erie occupied the territory south of and adjacent to the great lake which bears their name, extending roughly from its western watershed eastward across northern Ohio and Pennsylvania to the vicinity of the Genessee valley, in New York. They are described as a powerful, warlike group of the Iroquoian family, fairly populous and mainly

[1] Mooney and Thomas: under Algonquian family.

[2] Relation of 1655–56, Chapter XI, and scattering.

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sedentary, with numerous towns and settlements throughout the designated territory. For many years preceding 1650 they had been frequently at war with neighboring tribes, particularly with the Iroquois proper, and a few years later, at their principal stronghold near the city of Erie, Pennsylvania, they suffered a crushing defeat, amounting practically to annihilation, at the hands of the Iroquois warriors. Minor reverses at about the same time, and the overrunning of their territory by the victorious enemy, ended the career of the Erie as a people. Of the remnant who escaped death, some were taken prisoners by their conquerors and others found refuge with friendly tribes. All that remains to attest the greatness of the once powerful Erie is their name, as given to the great lake along which their territory lay, to the Ohio county of Erie and to the city and county of that name in Pennsylvania.

The completeness of the ruin of the Erie cannot but be taken as significant of what befell other native Ohio tribes which came in the path of the unbridled ambitions of the resistless Iroquois league. While history does not specify the names of the several Algonquian tribes displaced and scattered by the Iroquoian invaders, it may safely be surmised that they were mainly of the great Central division of the Algonquian family, occupying in a general way the country of the old Northwest Territory, and comprising, among others, the so-called Miami group of Peoria, Cahokia, and Kaskaskia, and the Piankashaw, Wea, and Miami proper. It is possible that the related Shawnee, the western group of which, even thus early, had found its way northward to the vicinity of the Ohio river, may have felt the force of the confederated invasion.

While none of these groups is identified with the authors of the Ohio mounds, nor is it known that they were at the time accustomed to erect mounds, the bare fact of their dispersal, aside from its historic interest, is significant. It is generally known that while many tumuli of the general mound area were made and used within the historic period, particularly in the Southern states and in the country to the north and west of Ohio, the mound-building trait had become obsolete in the Ohio area prior to contact between its tribesmen and white men. Apparently no authentic instances are

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recorded of the finding of objects of white manufacture in the mounds of Ohio, and the fact that this important mound region should be the exception in this respect might be taken as additional evidence of the completeness of the Iroquoian devastation. While it is possible that mound-building in the Ohio valley may have ceased from other and earlier causes, it would seem a reasonable surmise that the trait, though obsolescent, was still existent at the time of the Iroquoian invasion and that its extinction was completed by that event through the expulsion and scattering of any tribes in which it prevailed. The twin phenomena—the relatively long period of depopulation in so large and strategic an area, and the early disappearance of mound-building in the very center of its highest development—are markedly striking. The suggested Algonquian authorship of certain archaeological remains will be discussed in connection with prehistoric culture groups.

## Recognition of Culture Varieties

A tentative discussion of the prehistoric culture groups of the Ohio area does not permit of more than casual reference to the period of general exploration, covering the greater part of the past century, and a passing tribute to such pioneer investigators as Atwater, Whittelsey, and Squier and Davis, who blazed the archaeological trail in the territory; nor will space permit of anything like due consideration of the work of Putnam, Thomas, Moorehead, Fowke, Mills, and others, whose labors have brought the science in the local field to its present high stage of development.

While the existence in the Ohio and the general mound areas of diversified culture groups doubtless had been suspected by individual observers considerably earlier, an examination of the literature shows the first authoritative expression to have been that of Professor Cyrus Thomas, in 1891. As director of the extensive campaign of mound exploration inaugurated by the Bureau of American Ethnology in 1881, he concluded that the native Indian tribes were the builders of the mounds and earthworks, thus setting at rest, officially at least, the mooted question as to the identity of the so-called Mound Builders. Supplementary to this finding,

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he directed attention to the existence of various culture groups by declaring that

> the mound-builders . . . consisted of a number of tribes or peoples bearing about the same relation to one another and occupying about the same culture status as did the Indian tribes inhabiting this country when first visited by Europeans.[1]

This pronouncement, while primarily general in its application, extended equally to the Ohio country, as a part of the general mound area, and sounded the keynote to a new order of archaeological investigation.

In the meantime, Professor Frederick W. Putnam and Warren K. Moorehead, working independently of one another, had recognized and demonstrated the existence of two distinct culture groups in southern Ohio—an extensive village-dwelling culture, represented by sites at Madisonville and Fort Ancient, and a more highly advanced culture, at the Turner and the Hopewell earthwork groups. The general character of the two cultures was manifest, but the data at hand were not sufficient to fix the exact status of either nor to determine their geographical extent. Moorehead, in 1892, advanced the theory of the existence of two races in Ohio, a brachycephalic and a dolichocephalic, and expressed the opinion that the builders of the Hopewell works were "an advanced off-shoot to the north" of the Stone Grave people of Tennessee.[2]

The next reference to the subject is that of Gerard Fowke, in 1901, who wrote

> there is abundant evidence that any of the localities named (the general mound area) have been occupied by two or perhaps more different races; nearly everywhere appear aboriginal remains so diverse from one another as to make it almost certain that they belong to a different period of construction or to an unrelated people. Particularly in southern Ohio the dissimilarity to be observed in different remains which were at first thrown into a single classification denotes that several waves of population swept over this region . . . while the stone graves or cairns fall into a class to themselves.[3]

A definite beginning at classification of local culture groups was

[1] Thomas: p. 17 (II).

[2] Moorehead (2): p. 197.

[3] Fowke: p. 101.

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made when, in 1903, Professor William C. Mills suggested and used the names "Fort Ancient culture" and "Hopewell culture" as designating the two aboriginal groups represented by these sites respectively, which were the more nearly typical examined up to that time.[1]

Moorehead, in 1909, in "A Study of Primitive Culture in Ohio,"[2] recognizes the Fort Ancient and the Hopewell cultures, and suggests a third—"the Glacial Kame culture." Referring to the prehistoric sites of northern Ohio, he declares they are not Hopewell and hesitates to pronounce them Fort Ancient.

In a tentative outline of American culture areas,[3] Professor W. H. Holmes, in 1914, includes the northern portion of Ohio in his Upper Mississippi and Lakes region, and the southern part of the state in the Middle and Lower Mississippi Valley region. He points out that these areas, as outlined, are by no means complete culture units, but comprise many tribes, and that there exist a dozen or more somewhat localized centers of development.

Dr. Clark Wissler, in his plan of archaeological areas,[4] (1917) designates northern Ohio as pertaining to the Iroquoian area, and apportions the southern sections of the state to his Mississippi-Ohio area. He refers to the dominant Hopewell and Fort Ancient groups, and to Moorehead's suggested Gravel Kame culture. He characterizes the Fort Ancient as more extended and less specialized than the Hopewell, and finds the distinctive traits of the last-named more like those at the center of the Mississippi-Ohio area, in Tennessee. Dr. Wissler's classification, it should be noted, is not one of culture groups, but a broad continental outline of archaeological areas; and a similar specification applies to Professor Holmes' scheme. They are cited, however, as having an important bearing upon the local plan of detailed culture classification and as offering a good working perspective for the same.

The above references practically exhaust the literature relating to culture classification in the Ohio area. In addition to the several

[1] Mills (3): p. 95.

[2] Moorehead (3): pps. 137–150.

[3] Holmes (2): pp. 424–429.

[4] Wissler: Map of Archaeological Areas; fig. 76.

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culture groups mentioned therein, the writer wishes to offer for discussion what Professor Mills has considered the Adena subgroup of the Hopewell culture, and another, not heretofore specified—the Algonquian prehistoric culture. The proposed groups, then, are as follows: The Fort Ancient; the Hopewell; the Adena subgroup; the Stone Grave culture; the Iroquoian; the Glacial Kame, and the Algonquian group.

## The Fort Ancient Culture

Although the dominant one of several culture groups of the Ohio area and more extensive in its distribution than any other, the Fort Ancient culture was not the first to be recognized by writers and explorers. From the time of Atwater and Squier and Davis down to the closing quarter of the past century, the more striking archaeology of the Hopewell group practically monopolized attention, until the explorations of Putnam at the Madisonville site[1] and those of Moorehead at Fort Ancient,[2] directed attention to the importance of the culture represented thereat. For another quarter-century the Fort Ancient group continued to be identified exclusively with the Miami valley, where the above-mentioned sites are located. In recent years, however, the work of Professor Mills at such important sites as the Gartner Mound and Village and the Baum Village[3] in Ross county, and the Feurt Mounds and Village[4] in Scioto county, has demonstrated that the Scioto valley was equally frequented, if not more so, by this group. Across the Ohio in West Virginia and Kentucky, and lower down the river, particularly at Lawrenceburg and Aurora, Indiana, are to be found the sites of villages of this culture. The site examined by Smith in Mason county, Kentucky,[5] the only one of the group outside Ohio to be thoroughly explored, is similar in every respect to the Feurt site and, with some minor exceptions, to those in Ross county.

The full extent of the area of Fort Ancient occupation has not

[1] Low: Pts. 1–4.
[2] Moorehead (1).
[3] Mills (3).
[4] Mills (6).
[5] Smith.

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as yet been determined. In a general way their territory may be defined as embracing the valleys of the Miamis and the Scioto, southward from central Ohio to the Ohio river, and down that waterway, on either side, almost or quite to the Mississippi.

Although overshadowed by the Hopewell group, as regards esthetic development and geometrically formed earthworks, the Fort Ancient peoples have to their credit two earthworks, each of which is the most striking of its class in the mound region if not of the continent—Serpent Mound, in Adams county, and Fort Ancient, in Warren county—and the most extensive prehistoric village site, located near Madisonville, Hamilton county. Of the mounds proper, to this great culture apparently are to be attributed a larger percentage of the state's total of more than 3500 than to all other cultures combined, while the greater number of the so-called hill-top enclosures or fortifications are also attributable to the group. The mounds of this group vary from almost imperceptible elevations to those of medium and large size and are more or less conical in form. With but few exceptions they were purely mortuary in purpose. While they usually occur in proximity to camp or habitation sites, they are not uncommonly found isolated from other evidences of occupation.

Burial customs of the group were the simplest possible. When interment was made in mounds the bodies seldom were provided with prepared graves, but were hastily and carelessly deposited upon or above the base line. When interment was made below the original surface, graves, of course, were dug. Bark coverings, rarely stone-lined graves, with an occasional clay vessel or other personal possession, were the only material tributes accorded the dead. Burial outside the mounds was in shallow graves, either in designated burial grounds or in any chance spot, as beneath or adjacent to the domicile of the departed.

As an index to the culture complex of the Fort Ancient group, there may be enumerated these several traits, not altogether unique but at least characteristic, in a comparison with the remaining groups of the area. Village dwellers, preëminent; mound-building, extensive; abundant manufacture and use of pottery;

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free use of bone and antler, and work in stone and flint liberal, but not greatly varied. Of the characteristic objects of artifice there may be mentioned bone awls of uniform types, made from bones of the deer and wild turkey; draw-shave scrapers, made from leg bones of the deer and elk; triangular unnotched projectile points and blades of flint; stone celts of oval and rectangular section; and, particularly toward the south, discoidal or gaming stones. What may be considered as negative traits are: absence of or only occasional use of cremation of the dead; very limited use of copper, mica, obsidian, ocean shells and other materials from distant sources; absence of grooved stone axes, bell-shaped pestles and banner-stones, and relative rarity of notched flint implements.

To none of the remaining cultures of the state has there accrued so much data, valuable as evidence in comparison with other archaeological cultures and with historic groups, as to the Fort Ancient culture. Such comparisons show them to have possessed very little in common with the Algonquian peoples supposed formerly to have populated a great part of the state; and this is no less true with respect to the historic tribes and the archaeological areas to the south of the Ohio river. In an unexpected quarter, however, at least from the standpoint of historic evidence, we do find what appears to be an archaeological affinity—namely the Iroquoian area centering in New York state.

While the territory of the Fort Ancient group of southern Ohio and the lower Ohio valley, and the Iroquoian region of New York and its extension westward into Ohio, may be considered as fairly contiguous, the evidence of intercourse between the two areas probably is not sufficient to indicate anything more than a possible diffusion of culture traits. The area separating the two groups, while restricted as to extent seems fairly pronounced, with little evidence of overlapping or intermingling. On the other hand, since the Iroquoian people are supposed to have originated southward,[1] "down the Ohio river" from their principal historic area, and since the Fort Ancient area trends to the southward, just how far being as yet undetermined, the possibility presents itself of the two

[1] Parker (2): p. 507.

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groups having been in contact in the far southland in very early times.

The extensive studies of Iroquoian archaeology by Beauchamp, Parker, and others, and those of the Fort Ancient culture by Moorehead, Mills, and Smith, make possible a very full comparison of the two groups. While such comparison may not indicate definite relationship, it appears to the writer that the number of traits possessed in common and the general similarity of the culture status of the two, is rather marked. Both groups have left behind them numerous and extensive habitation sites, the character of which is strikingly similar. Both practised the building of mounds for mortuary purposes and of earthworks of a defensive nature. Both were extensive potters, and while their respective products differ in some respects, they are strikingly alike in others. The typical earthen pot of the eastern and southern Iroquoian area bearing the rectangular extended rim, is not found in the Fort Ancient area; but, on the other hand, the typical Iroquoian pottery decoration, consisting of parallel incised lines in contiguous triangular fields, is found in the Ohio area, while the Algonquian dental or roulette decoration apparently is found in neither. Both groups utilized bone and antler to a marked extent, and in much the same manner. The triangular unnotched projectile point is characteristic of each, as is the celt, or ungrooved axe, while the grooved axe, the bell-shaped pestle, certain problematical and other forms, are conspicuously absent.

Whether these resemblances are to be taken as indicating relationship, or even a considerable diffusion of culture traits, from proximity and vicinage; or whether merely a rather marked instance of similar independent development, must be decided by a more careful and detailed comparative study of the two groups.

## The Hopewell Culture

In practically all the earlier literature—in fact, down until very recent years—the mounds and earthworks of the Hopewell prehistoric culture so monopolized discussion as to be practically a synonym for mound-building, and so striking are their character-

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sitic traits that it was late in the past century before the less imposing culture groups were even accorded recognition. While but few traits can be said to be unique to any given group of Ohio aborigines, the following may be taken as characteristic of and as nearly as possible peculiar to, the Hopewell culture:

Extensive complex earthworks of geometric forms; mounds usually low, irregularly shaped structures, often within or adjacent to the typical earthworks; burial tumuli usually cover the sites or remains of structures, varying from unpretentious enclosures of upright timbers or posts to similarly constructed buildings of large size, serving as sacred places into which the dead were taken for funeral obsequies and sepulture; cremation of the dead and disposition of their ashes in prepared graves of puddled clay the rule, though not to the exclusion of uncremated burial; sculptural art highly developed, particularly in the carving of life forms in stone; comparatively high development of the textile and fictile arts, as evidenced in woven cloth and fabric and in burned clay pottery ware; strikingly free use of copper, both for ornament and utility; and the extensive possession and use of materials from distant sources of supply, as mica, obsidian, quartz crystal, and galena.

Dr. Clark Wissler, in his classification of archaeological areas[1] is inclined to accord the Hopewell culture a place marginal to the culture complex centering in Tennessee. This classification, as before noted, is one of areas rather than of cultures, and in its broadness of scope does not permit of the close analysis to be had in a local handling of the subject. Nevertheless, since it makes specific mention of the Hopewell group and tentatively correlates it with a definite culture center outside the assumed area, it very properly may be considered as pertinent to the present subject of discussion. From the local viewpoint, and in the light of recent explorations, the Hopewell group would appear to merit a more prominent position than that accorded it in the general continental classification above referred to. Comparison of the distinctive traits of the Hopewell with those of the culture centering in Tennessee, or with any other archaeological culture complex, will

[1] Wissler: p. 252.

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demonstrate the striking individuality and specialized development which they possess. The extent to which the peculiar Hopewell traits appear in the area south of the Ohio river, and *vice versa*, is very meager; in fact, only such as would naturally result from intrusive entry through tribal intercourse, commerce, and other means of diffusion. Of the objects characteristic of the dominant culture of the southern area, namely, pottery with color decorations and modeled in life forms; large finely made discoidal stones; large pipes in animal and human forms; engraved gorgets and "hairpins" of shell; repousse copper plates; large chipped flint implements; and reel-shaped and spool-shaped objects of copper—only the last named can be said to be anywhere near to common occurrence in the areas to the north and south of the Ohio river.

Moorehead sees strong evidence of southern origin for the Hopewell group,[1] basing his belief mainly on the great cache of flint disks found in a mound of the Hopewell works. This flint, he states, appears to be from quarries on Little river, Tennessee. Taken alone, this item of evidence would appear as significant; but as later explorations have shown that the group made free use of similar nodular flint from the deposits of southern Indiana, and in view of the fact that they had extensive recourse to many materials from distant sources of supply—as obsidian, obtainable in a wide area of the western country; copper, from the Great Lakes region; mica, quartz, and steatite from the southeastern states—it would hardly appear to be of definite value.

Professor W. H. Holmes, considering only the pottery of the group, finds that the product of the Hopewell ceramic art belongs to his Northwestern group.[2] This slight indication appears to be of definite value, since similar resemblance with respect to other artifacts of Hopewell use is found throughout the extent of a strip of territory extending northwestward from Ohio, across Indiana and into Iowa. This resemblance, applying to pottery-ware, pipes, and some other minor relics, would seem to point to archaeological relationship or affinity, or to indicate a northwestward trend

[1] Moorehead (2): p. 145.

[2] Holmes (1): p. 188.

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of the culture centering in Ohio. While comparatively little pottery of the Hopewell group has been found, sufficient has been taken from the various mounds explored to show that it is decidedly superior to the ware of the Fort Ancient culture, and perhaps but little inferior to the pottery of the southern area, although much more restricted as to variety and use. The characteristic decoration, aside from stamped designs is the dental, or zig-zag roulette marking, corresponding exactly to the markings found on the pottery of the New England archaic Algonquian group. This slight clue may be significant, since the design is characteristic of much of the eastern Algonquian pottery, but probably is not found in the Iroquoian ware.[1]

The deductions of Professor Cyrus Thomas, who found evidence in the then available data for a very strong argument in favor of Cherokee origin of the Hopewell mounds, may be passed over at this time, since more recent developments have changed the character of the evidence. However, in view of the present knowledge with respect to the fact that many Hopewell mounds cover the sites of pre-structures or buildings, it is interesting to note Thomas' statement that "In the south, houses were built upon mounds, then burned and mounds erected over them."[2] The same strong resemblance in this particular direction is found in the record that "Cherokee ceremonial houses, or temples, for sun worship, in which were perpetual fires, (were) set upon mounds."[3] The strong analogy of these Cherokee ceremonial proceedings to the ceremonial structures covered by the Hopewell mounds, in which even the perpetual fires appear to have been kept, is most striking.

With deference to opinions based upon early explorations, it can be said that the Hopewell culture group, from the more recent evidence, appears to be a definite and distinctive variety, with but little apparent relationship to any known group of the native race or to any observed archaeological area, and that scant evidence has been adduced that can be taken as indicative of its origin or

[1] Willoughby: p. 84.
[2] Thomas (6): p. 17.
[3] Wissler: p. 223.

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earlier habitat. The more important exceptions are those above noted, namely, the extension northward of artifacts apparently of Hopewell manufacture, probably denoting migration in that direction of bands marginal to the Hopewell proper in Ohio, rather than indicating northwestern origin of the group; the use of the Algonquian pottery decoration, dental or roulette; and the resemblance with respect to ceremonial structures as between the Hopewell and the Cherokee.

The Hopewell group was resident in the Scioto valley, from Columbus southward, with important isolated seats at the headwaters (Newark) and at the mouth of the Muskingum; along the lower course of the Little Miami and on the site of Cincinnati, with some evidence of occupation across the Ohio river to the south; and along the belt extending northwestward from Ohio, across Indiana and Iowa.

## The Adena Group

One hesitates to characterize the builders of the Adena mound, of Ross county, and of other similar tumuli, as representatives of a distinct culture; yet the traits displayed therein appear sufficiently distinct to indicate, if not to warrant, such a classification, particularly from the localized point of view. Much depends, of course, on the interpretation given the term culture, or culture group, a designation so elastic in its application that it may be construed as indicating a few broad and well-defined social divisions or, as it is here employed, it may be taken as almost synonymous with the terms tribe or nation, as of historic use. Of such minor divisions there doubtless were a number in the Ohio area in prehistoric times, and it is in this localized interpretation that place as a distinct culture variety is bespoken for the Adena group. In his report of the exploration of the Adena mound,[1] Professor Mills makes no attempt to designate the culture to which it belongs, but with the fuller evidence afforded by the Westenhaver mound and after consideration of the data pertaining to various others of the same type he is inclined to fix their status as of the early Hopewell

[1] Mills (2).



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culture. In his report on the Westenhaver mound, Professor Mills concludes:

> The examination of the Westenhaver mound shows that it belongs to the early Hopewell culture, and in many ways resembles the Adena mound. . . . With evidences of cultural advancement so obviously lacking, as regards the aboriginal inhabitants of the Ohio valley, it is perhaps gratifying to note that here, at least, in tracing the history of the Hopewell culture, we have something very definite. The evolution from a lower to a higher plane is exemplified in the Adena and Westenhaver stages, with such mounds as the Harness and the Seip intermediate, and the Hopewell and Tremper mounds representing the highest development.[1]

The distinctive traits of the culture represented by the Adena type of mounds may be stated as follows: Shapely, conical mounds, generally single but sometimes occurring in apparent series; mounds unaccompanied by earthworks; absence of indications of pre-structures of upright timbers; sites of mounds unleveled and showing no evidence of previous use; erection of mounds often begun by piling logs and brush upon the sites or bases; non-cremation of the dead; burial made upon the base line and throughout the mounds, usually with an important central grave below the base line; sepulchres of logs often used, particularly for the more important burials. Materials from distant sources, as with the Hopewell culture proper, were extensively used, but copper appears to have been employed for objects of ornamentation only, and rarely if ever for utility implements. Of the artifacts persistently occurring there may be mentioned copper bracelets and finger rings; gorgets of the expanded center and concaved edge type; tubular pipes; necklaces of beads made from univalve shells; and projectile points of flint of the ovate unnotched and the stemmed types.

While the affinities of the Adena type of mounds are apparently strongly with the Hopewell culture, and their classification as such, in a marginal sense, doubtless is justifiable in a broad scheme of handling, there are many fundamental differences between the traits of the two groups. Aside from the use of copper and other material from distant sources, very few traits of the Adena type will be found to correspond in any degree to those of the Hopewell type.

[1] Mills (5): p. 284.

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In the dearth of culture horizons, stratigraphic and other evolutionary evidences in the Ohio area, it would be gratifying to find that the Adena type of mounds represents an earlier phase of the Hopewell culture; but if this should prove to be the case, we must suppose a very considerable period of time necessary for the Adena people so completely to change their distinctive traits, and to evolve into the typical Hopewell culture variety. Evidences of long-continued habitation in the area, necessary to such a change, naturally would be expected to manifest themselves as examination of the tumuli proceeds. Aside from their apparent affinity with the Hopewell, the Adena mounds do not suggest relationship to any outside archaeological area or historic tribes.

Aside from the Adena mound, taken as the type, and the Westenhaver mound, a number of others, very similar in character, have been examined within the past half-century. They occur principally in Ross and adjoining counties, and mainly in the Scioto valley. From what is known of the great Miamisburg mound, in Montgomery county, the largest mound in Ohio, it appears to be of the Adena type. Explorations of the Grave Creek mound,[1] the Great Smith mound,[2] and others of the Kanawha valley of West Virginia, strongly indicate that the Adena culture was not confined to the Scioto and Miami valleys of Ohio, but that it extended well across the Ohio river to the southeast.

## The Stone Grave Area

In the so-called Stone Grave area of southern Ohio is to be found evidence of a culture variety apparently distinct from any other of the state, but hardly of sufficient importance or extent to merit more than passing reference. The area extends for approximately twenty-five miles along the Ohio river in the Ohio counties of Brown and Adams, and correspondingly across the river in Kentucky. The tumuli of the section, located upon the high hills and ridges overlooking the valley, are in the nature of cairns, or mounds of stone and earth, containing graves, which usually are

[1] Tomlinson.

[2] Thomas: p. 425.

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lined with slabs of stone. These remains are very primitive in character and although they have received considerable attention from the explorer, nothing which would seem to throw light upon the identity of their builders has appeared. But few relics are found with the burials, and the skeletons almost invariably are badly decomposed and scattered. A few artifacts taken from the graves suggesting the high culture groups to the north and south, can hardly be taken as indicative of the culture of the stone grave builders, but rather impress one as being intrusive thereto.

Dr. Cyrus Thomas was inclined to attribute the stone graves of southern Ohio to the Shawnee,[1] although his explorations uncovered many of them in supposed Cherokee sites on Little river, Tennessee. The best study of these peculiar tumuli is that of Fowke, who, after a careful and extended examination of the region, concluded:

> It is impossible to assign a date to these graves or to determine what tribe of Indians may have constructed them . . . while the Shawnee method of setting slabs on edge around a body was largely followed in this locality, there are also found here radical departures from any known Shawnee graves. This may be due, however, to local customs slowly developed during a long period of quiet, unmolested occupation of the limited area where these cairns are found. The copper "spool-shaped" ornament and the flat-stemmed pipe (found in the stone graves) are objects which are commonly considered as pertaining to the "Mound Builders," but this people was certainly not concerned in the stone graves of this portion of the Ohio valley.[2]

Stone graves are by no means confined to the district above mentioned. They occur not infrequently throughout Pike, Brown, and Highland counties, in the counties adjacent to the headwaters of the Muskingum, and were found by Moorehead in burial sites at Fort Ancient, Warren county. The stone graves at the last mentioned place, as shown by Moorehead, appear to be distinct from the Shawnee stone graves of Tennessee and Kentucky. Stone graves or cists are not uncommon in the mounds of eastern and southeastern Ohio, particularly in the territory occupied within historic times by the Delaware Indians, to whom they are attributed by Thomas.[3]

[1] Thomas: p. 697.

[2] Fowke: p. 405.

[3] Thomas: p. 697.

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Taking the situation as a whole, a logical deduction would seem to be that the area along the Ohio river represents a distinct community, possibly a retrograde off-shoot from the Shawnee, or at least an isolated group or tribe which had borrowed the custom of burial in stone graves from the Shawnee. In the case of the Fort Ancient peoples and of those further east and north, the use of stone graves easily may have been due to culture diffusion or borrowing; in all instances, it should not be overlooked that the use of stone in burial is a very natural thing, and that its presence and ready accessibility might easily suggest its use.

## The Iroquoian Area

The archaeology of the Iroquoian family in Ohio has received almost no attention, either from writer or explorer; yet it is perhaps as sharply defined as any other of the prehistoric culture varieties of the state, at least in so far as territory occupied, earthworks and habitation sites, and characteristic manufacts, are concerned. Roughly speaking, the Iroquoian tribes may be said to have embraced the territory comprising the northern one-third or more of the state, bordering Lake Erie, their occupancy being more pronounced toward the east, whence it extended eastward into Pennsylvania and New York. The tribes occupying this Ohio territory pertained to the Huron-Iroquois stock, and presumably were mainly those of the Erie, or Cat nation, and possibly of the Seneca, the westernmost nation of the Iroquois proper.

What little attention the Iroquoian archaeological area in Ohio has received has been incidental to the study of the area as a whole and, historically, in connection with the story of the Erie nation and the invasion of their country by the Iroquois confederation. The scant knowledge of its archaeology is such as may be obtained through comparison of relics and remains with those depicted in the writings and reports of Beauchamp, Parker, and others. From Parker's thorough studies of Erie and other Iroquoian sites in New York state,[1] we are led to deduce that certain sites in northeastern Ohio will prove to be those of the Erie; at the very least, it

[1] Parker (1).

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is clear that they are Iroquoian, and that they hold the key to the archaeology of the area in question.

Exploration of this field, planned by the Ohio Archaeological and Historical society for the near future, doubtless will result in clearly defining the archaeology of Iroquoian occupation of the state and the differentiation of the various groups responsible therefor.

## Glacial Kame Burials

In considering the "Glacial Kame culture," suggested by Moorehead in "A Study of Primitive Culture in Ohio,"[1] it should be noted that his observations are confined to southern and central Ohio, while the present study aims to comprise the state as a whole. Moorehead classes the people or tribe to whom the glacial Kame burials pertain as representing a culture distinct from the recognized Hopewell and Fort Ancient groups, and thinks it possible, though improbable, that they later became the carriers of the Fort Ancient culture. As typical artifacts of these burials he cites "tubular pipes, cannel coal ornaments, long slender union-shell gorgets, tubes of slate and hematite plummets." He very plausibly believes that glacial knolls suggested themselves to early man as burial places, owing to their prominence and the minimum of labor required in excavating graves, and, later, the construction of artificial mounds.

Supplementing Moorehead's observations with more recent data, and considering the state as a whole, the evidence appears about as follows:

Burial in gravel deposits and knolls occurs generally throughout the state where such elevations exist, but more particularly in the western part, notably in the glaciated sections lying west and north from its center.

These burials appear to pertain to any or all of the several cultures of the state, thus indicating that this form of interment cannot be taken as a trait peculiar to any one, but rather as a practice so natural as to be common to all. However, many of those observed by the writer appear to pertain to the Algonquian tribes which inhabited the region.

[1] Moorehead (3).

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Naturally grave burial, of which the gravel kame burial is one of several closely analogous types or variations, obtained more generally among those tribes in which mound-building was least practised, although it is unreasonable to suppose that those who carried mound burial to its highest development disposed of all their dead in that manner. The "high places" of the earth have appealed to human kind through all time and in all parts of the world as places of interment and worship, and their utilization by the Ohio tribes is by no means unique.

## The Algonquian Culture

Notwithstanding that approximately two-thirds of the area of the state of Ohio, at the time of the Iroquoian conquest, is supposed to have been populated by tribes of the Algonquian linguistic stock, no attempt appears to have been made to indicate their archaeological remains within the territory. This may be due, in part, to the overshadowing interest evoked by the more evident mound-building cultures and their tumuli; at any rate, little cognizance seems to have been taken of the fact that other groups of aborigines, to whose culture the building of mounds was foreign or only incidental, doubtless were resident throughout the greater part of the area.

The culture which we here presume to designate as the Algonquian, and the characteristics of which we shall attempt to outline, in so far as may be done from minor remains, appears to have been widespread in its occupancy of the state, embracing not only the territory of the mound-building cultures, but practically all habitable territory adjacent thereto.

While data respecting the Algonquian culture are not numerous, among the distinctive traits applying to their prehistoric ethnology, as cited by Wissler,[1] may be mentioned the following: a rather weak development of pottery; work in stone and bone weakly developed; and probably considerable use of copper. According to the same author the grooved stone axe, the elongated pestle, and the bannerstones and other problematic objects, are to be attributed

[1] Wissler: p. 221.

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to the Algonquian family. The first-named trait, respecting pottery, seems to be fully borne out in the sites and burials apparently attributable to the Algonquian group in Ohio. The pottery, while fairly in evidence, seems to be mostly plain and confined to cooking vessels and containers. Work in stone, on the other hand, appears to be strongly developed, although the use of bone, from the limited data available, seems to have been restricted. From the rather common occurrence of copper in supposedly Algonquian burials throughout the glaciated section of the state, and where mounds are least numerous, a considerable use of that metal is indicated. As to the remaining artifacts cited as pertinent to the culture—the grooved axe, the roller pestle, and the bannerstone and allied problematic forms—all are found in abundance in connection with the suggested Algonquian occupancy and, as before noted, are noticeably absent from sites attributable to the two predominant mound-building cultures of the state.

For the study of the Algonquian group, those sections of the area where evidences of occupation by the established mound-building cultures are least in evidence, naturally are most suitable. Throughout central and western Ohio and farther north, many collections of surface specimens have been made, from those containing a few specimens to those of unusually large size. Examination of a number of these shows the artifacts of which they are composed, with respect to relative occurrence, as follows: chipped flint specimens of the notched and stemmed types; celts, or ungrooved axes; chipped flint specimens of the triangular unnotched type; grooved axes; gorgets or tabular specimens; bell-shaped and roller-shaped pestles; grooved hammers; bannerstones and related problematical objects.

Of these several classes of artifacts, the celts and the gorgets appear to be common to the various cultures of the state; the notched or stemmed flint specimens are common to the Hopewell culture, but are the exception in the Fort Ancient group, where the triangular unnotched form is typical, almost to the exclusion of the notched form. Examination of the literature covering the exploration of upward of two hundred Ohio mounds shows but one authentic

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instance of the finding of a grooved axe.[1] In the villages of the Fort Ancient culture, three grooved axes were taken from the Baum village,[2] and a few specimens were found at Fort Ancient.[3] The mound above referred to, aside from the presence of the grooved axe, presented other features indicating Algonquian origin. The few axes taken from the village sites easily may have been intrusive or pertinent to Algonquian occupancy or presence, rather than to that of their residents proper. The bell-shaped and the elongate pestles seem to be entirely absent from the villages of the Fort Ancient peoples, and the only instance recorded of the finding of this implement in the mounds of the Hopewell group is a single bell-shaped specimen taken from the Tremper mound in Scioto county.[4] Grooved hammers and bannerstones, in so far as the writer is informed, have not been found in Ohio mounds, outside the Iroquoian area, at least, although the problematical forms are reported as mound finds outside the state.

It may be said without fear of disproval that those areas of the state where mounds are less in evidence have produced proportionately as many surface specimens as have the regions where mounds are more abundant. However, the types of artifacts found in the first-named districts are mainly those generally recognized as belonging to the Algonquian family. It is true that these specific types are found also, and in considerable numbers, in the mound districts, in which case they apparently represent the presence of Algonquian tribes rather than the handiwork of the mound cultures proper. Their presence naturally tends to increase the number of specimens found on the surface in the mound districts.

The several distinct and highly specialized types of artifacts above mentioned are, as we have seen, so unusual in the mounds and sites of the two dominant mound-building cultures as to show that they cannot be attributed thereto, as types; yet they are objects of everyday occurrence in the cultivated fields over a great part of

[1] Mills (1): p. 321.
[2] Mills (3): p. 42.
[3] Moorehead (1): p. 57.
[4] Mills (4): p. 234.

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Ohio. The significance is so clear, it appears to the writer, as to admit of only one deduction; namely, that these several types pertain to a culture variety distinct from those already recognized, and which logically may be identified as the Algonquian group. It is probable that mound-building, while of restricted practice, was not altogether foreign to this group. Their habitation sites, while not so extensive as those of the Fort Ancient culture, appear to be fairly numerous, but up to the present time they have received scant attention from the investigator. The writer has observed several sites where notched flint specimens appear to occur to the exclusion of the unnotched variety, and where grooved axes and bell-shaped pestles are found in significant numbers.

Future explorations, it is believed, will clearly define the presence and extent of occupancy of this group of the Algonquian family in Ohio.

## Time Relationship of Ohio Culture Groups

While originally the sole aim of this paper was to differentiate the apparent culture varieties of the Ohio area, in the end this procedure has seemed to entail the obligation of brief reference to the matter of time relations as among these several groups.

Since the native American race, as historically observed, is characterized by numerous cultural divisions and these by frequent changes of habitat, it may be inferred that the Ohio area, throughout the centuries preceding discovery, sheltered a proportionate number of culture groups. Interpretation of the time relations of these several groups is a problem toward which the student of anthropology turns an inquiring mind, and an important task awaiting archaeological research in Ohio. Unfortunately, much of the earlier investigation in the area is of little value in this connection and the work of more recent years has not covered sufficient ground to furnish the cumulative evidence necessary to a solution. At the normal rate of progress, years of systematic research are needed for this; and the danger of drawing general conclusions from incomplete evidence must be avoided. Nevertheless, a few indications may be cited, not necessarily as conclusive in themselves, but as suggestive of fact.

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Merely as a tentative working hypothesis, then, the following chronological scheme for the culture groups of the Ohio area, as indicated by the available data, is offered: The Algonquian group were the earliest, the most persistent and widely distributed, and the last to disappear, of Ohio's prehistoric peoples, and, therefore, were contemporaneous with all other groups present in the area; the Fort Ancient and the Hopewell groups were contemporaneous one with another, and their presence in the area lay within the limits of appearance and disappearance of the Algonquian group; the Adena group has not been sufficiently examined to produce evidence as to their time relations with other groups; the evidence for the Stone Grave group points to contemporaneity with the Hopewell; and the Iroquoian group, arriving in the area in late prehistoric times, disappeared about the middle of the seventeenth century.

In the main, the indications supporting these suggestions have been cited in the preceding pages; the remainder, with the exception of those which are self-evident, are supplied in the following paragraphs dealing with the several groups.

*Algonquian.*—The wide distribution of this group over the general area of which Ohio forms a part, and their prolonged occupancy thereof, are shown by an equally wide distribution and abundance of archaeological remains and, in the first-named respect, by historic record as well. In view of these facts and of the indications which follow, the Algonquian group properly may be considered as the most representative people of the area and as the prototype of its primitive culture groups. The archaeological remains of the group, as cited in a preceding page, have been found to underlie those of both the Iroquois and the Erie in the main Iroquoian area, centering in New York state, where they appear to be the earliest evidences of human occupation. The writer's observations are that certain artifacts found scattered through the earth composing some of the mounds of the Fort Ancient and the Hopewell groups and on the original surface covered by them, are Algonquian; however, sufficient opportunity to verify this surmise has not as yet presented itself.

Historical and archaeological evidences indicate that the Algon-

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quian peoples constituted a generalized rather than a highly specialized group, consisting of numerous tribes of unusually nomadic habits. They appear to have been present at one time or another in practically every habitable part of the area, but developed no well-defined centers of localization. Under such conditions it would not be particularly difficult for a highly specialized people, as the Fort Ancient or the Hopewell, to gain and maintain a footing within their territory, regardless as to whether or not their action might encounter opposition. An actual instance of such a movement is that of the Iroquoians who, from their early home to the southward, penetrated the territory of the Algonquian family which, amoeba-like, drew back upon itself and proceeded to surround the invaders, but was unable to absorb them. That the main body of Algonquians, despite their long residence in the area, failed to reach as high a plane of development as did some others is no anthropologic anomaly; moreover, it would not be illogical to suggest that one or more of the advanced groups, as the Adena and even the Hopewell, may have been progressive offshoots of the Algonquian stock. Popular belief in the greater age of the so-called mound-builders is very persistent; but recognition of the fact that as a rule a ruder stage may be supposed to have preceded a more highly developed status of culture, favors Algonquian precedence of occupation.

As to the late disappearance of this group, the evidence again is both historical and archaeological. The historical evidence need not be repeated. The archaeological indications are comprised in the fact that on a number of habitation sites of the Fort Ancient culture there are to be found numerous relics of later occupation by Algonquians. This is well illustrated in the case of the Feurt village site, in Scioto county, where many typical Algonquian relics, such as grooved axes, bell-shaped pestles and large deep-notched flint spear-points, were collected from the surface prior to its exploration. None of the types in question were found in the graves and refuse heaps of the site proper, nor do they occur, except intrusively, in other examined sites of the culture.

*Fort Ancient and Hopewell.*—Contemporaneity of these groups

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seems to have been definitely established, although the order of their appearance and disappearance remains to be determined. In 1904, Professor Mills, incident to the exploration of an important Hopewell tumulus (the Harness mound, in Ross county) examined a small mound adjacent thereto,[1] which proved to be Fort Ancient in its origin. However, near the top of the mound he found, intrusively, a typical Hopewell cremated burial. Obviously, the Fort Ancient had preceded the Hopewell at this particular place. But, in 1915, in exploring the Tremper mound (Hopewell) in Scioto county, several intrusive Fort Ancient burials, with characteristic implements, were found in the top of the tumulus.[2] Thus, so far, the evidence as to priority of occupation, for the Ohio area as a whole, stood at neutral; but, while in a sense disconcerting, this neutralized evidence was suggestive of fact in another direction—namely, that the two groups were contemporaneous in their presence. Further evidence in support of this surmise was forthcoming when, in 1916, the Feurt mounds and village site, directly across the Scioto river from the Tremper mound, were explored. In this extensive Fort Ancient site were found copper implements and ornaments of typical Hopewell manufacture[3] and corresponding in every respect with those taken from the Tremper mound. Comparison of the copper objects from these two sites, each so typical of the highly contrasted cultures which they represent, could result in no other reasonable conclusion than that the two sites were occupied simultaneously. Other similar instances, almost as striking in their significance, have been recorded, but need not be cited here.

The indications as to time relations for the Adena, the Stone Grave and the Iroquoian groups have been noted previously.

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# GUSTAF RETZIUS

By N. C. NELSON

ANTHROPOLOGY recently lost a distinguished worker in the person of Gustaf Retzius, Emeritus Professor of Anatomy at the Caroline Institute in Stockholm, who died July 21, 1919, in the seventy-seventh year of his age.

Sweden, during the last two centuries, has given to the world several names that must forever remain inscribed in the annals of science. Whether Gustaf Retzius belongs to this group only time can tell; but, certainly, it is seldom given to any single individual to render service to science on so monumental a scale and at the same time service of such uniformly high quality. Primarily a pioneer in modern medical research and in the development of histological technique, he found time also to contribute important works on physical anthropology, one brief paper being a description of the skeletal material obtained by G. Nordenskiold from the cliff-dwellings of the Mesa Verde in Colorado. It is fitting to recall also that his father before him—Anders Retzius—was an enthusiastic anthropologist and that he too wrote several brief papers on American subjects.

The following intimate remarks on the career of Gustaf Retzius are based mostly on accounts in the Stockholm papers for July 22, and largely on the appreciation penned by Prof. Carl M. Furst, one of his oldest students as well as his lifelong friend and co-worker.

Gustaf Retzius was born in Stockholm in 1842. His family belonged to the learned aristocracy of Sweden, there being three generations of naturalists behind him on his father's side and several men of science on his mother's side as well. His father, Anders Retzius, himself a noted anatomist, was a born genius, always bubbling over with original ideas, few of which however were carried to completion. Growing up in a stimulating atmosphere of this kind it was but natural that the young Retzius should follow in



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his father's footsteps. Accordingly, he graduated in medicine in 1871 and in 1878 was appointed Professor of Anatomy and Histology in which capacity he continued active with but few interruptions to the time of his death. Aside from his successful labors as a teacher, his scientific investigations range over the wide field from protoplasm to human craniology, with special attention as a rule to histology or minute structure. He has contributed more or less voluminous monographs, *e.g.*, on protoplasm, on spermatozoa, on the eye, the ear, the brain, and the nervous system, in addition to his more strictly anthropological papers. His biological publications alone comprise nineteen folio volumes with copious illustrations, many of them in his own hand. Altogether he left behind approximately five hundred important titles, two hundred and fifty of which are listed in his memorial volume.

The extraordinary volume of Retzius' labors is explained in part by the fact that he was able to finance his own publications, which he put out in superb style. Another explanation was his ability to inspire others to assist him, including both his wife and his mother. Back of it all, however, was his own genuine love for work.

One of the interruptions in his scientific career was the interval of 1884–87 when as editor-in-chief of one of the Stockholm dailies, *Aftonbladet*, the idealistic side of his nature had free play. As a young man he had tried his hand at poetry, had in fact won the Academy prize for a collection of sonnets. With his sister he had also translated and published many of Burns' poems and in later days he found time to compose cantatas, as for example on the occasion of the Linnaeus Celebration in 1907. In his new capacity as editor he took hold of a financially and politically bankrupt journal and in three years transformed it into a strong thoroughly progressive sheet. To indicate his liberal attitude it will be enough to mention that he did the unheard of thing of appointing a woman to the staff of the foreign department. For the rest he solicited articles from the ablest and most prominent men and women of the day. All social and humanitarian as well as pedagogical and scientific questions were presented. Art and literature likewise

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received due share of attention. For himself Retzius wrote a series of biographies, travel sketches and popular scientific articles; but he tried his hand also at political leaders, literary notices and when necessary delivered small talks and poems. For a time it looked as if he would let slip his scientific interests.

To a man of such gifts and such industry the highest honors and recognitions came as a matter of course from every quarter of the globe. Retzius was perhaps less well known in America than was his due; yet he had traveled here and was an honorary member, *e.g.*, of the Washington and Philadelphia Academies of Science. His last and most prized reward came to him from the Swedish Academy of Science, on the occasion of his seventieth birthday, in the shape of a memorial volume consisting chiefly of anatomical studies.

Although the writer was not personally acquainted with the deceased, he may venture to pay his own respects by referring once more to Retzius as an anthropologist. His interest in the subject was unquestionably instilled by the father, who is the recognized founder of modern craniometry. The elder Retzius died in his prime, in 1860, leaving a number of scattered brief papers. These papers, four years later, were gathered together and published with a foreword by the son as the first evidence of his interest in the subject. He was only twenty-two years of age at the time. In looking over the volume, it appears that the father as early as 1842 had made a beginning in classifying the human races on the cephalic-gnatic index basis and that in 1860 he presented before the Swedish Academy a map of the world showing the cephalic index distribution—a map which in all general respects is identical with that published by Ripley in 1899. On the basis of these investigations Anders Retzius became the first to recognize the mixed character of Europe's population and thus to challenge the validity of the Aryan hypothesis.

The most noteworthy publications by Gustaf Retzius himself commence with "Finnish Craniology" (Swedish, 1878), a title which covers in fact a considerable sketch of Finnish culture in all of its phases past and present, besides a brief chapter on the sup-



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posed former distribution of Lapps in Finland. The somatic division of the treatise includes observations and measurements on ninety-two living subjects leading to the recognition of two essentially different race elements as having entered into the Finnish population proper. Approximately ninety skulls were also obtained. For eighty of these he calculated the cephalic index merely while thirty of them were subjected to a more complete series of measurements.

The next work, "Ancient Swedish Crania" (Swedish ed. 1899, German 1900), is a well rounded, sumptuously illustrated report on somewhat more than one hundred skulls, fairly evenly distributed over the Neolithic, the Bronze, and the Iron ages. The results prove that the population of Sweden from the earliest times has been overwhelmingly dolicocephalic but that all along there has been present a slowly increasing admixture of brachycephalics, the exact origin of which is uncertain. The report proper is preceded by a valuable review of the general investigation of Europe's past and present racial characteristics.

The last important contribution, a joint work entitled, "Swedish Anthropology" (German ed., 1902), is a statistical study of army recruits. It was a labor of love, done with governmental sanction but at private expense. The investigation covers measurements and observations on the army contingents for 1897 and 98, in all about 45,000 subjects of the age of twenty-one. The general results show that the so-called pure Nordic type—tall, dolicocephalic, light hair and blue eyes—which constitutes more than ten percent of the entire population, is numerically strongest in what may be roughly designated as the interior central section of Sweden and that it becomes rarer towards the coast and also northwards and southwards owing to intermixture of other race types.

In conclusion it will be of interest to remark that Retzius was somewhat concerned as to the ultimate fate of his pure blond race. As is made evident in his Huxley lecture of 1909, he seemingly took the view of certain German writers that the North European race branch has for some thousands of years been slowly but steadily yielding ground to the short, dark, brachycephalic race branch, at

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home perhaps originally in Asia but for a long time dominant also in central and southeastern Europe. Furthermore, it was his opinion that the Nordic temperament is not adaptable to the coming industrial type of civilization. But whether or not Retzius was temporarily blind to the fact that in the struggle for existence under the new order of things the qualities of character commonly associated with the North European will still be in demand, he was thoroughly sensible of the importance and also of the delicacy of the whole question involved, and one cannot but feel that in these social and political aspects of anthropology he would have been a safe and sane guide.

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# BOOK REVIEWS

## METHODS AND PRINCIPLES

*Africa and the Discovery of America.* Volume I. LEO WIENER. Philadelphia: Innes & Sons, 1920. 290 pp. 19 pls.

This unquestionably interesting but in many ways unfortunate volume presents the reviewer with something of a puzzle, for a careful reading leaves one in doubt as to whether the author really intended his work to be taken as a serious contribution, or has attempted to perpetrate a rather elaborate jest. For while he has brought together material of much interest and arrives at startling conclusions, there is, especially in his later chapters, so much in the way of unsubstantiated assumption, hasty correlation, false reasoning, misunderstanding and misrepresentation of sources and evident lack of familiarity with the results of American archaeology that it is difficult to take the volume seriously.

Professor Wiener is concerned to prove two main theses: (1) the unreliability not to say "forgery" of much of Columbus' and other early writers' accounts of the New World, together with the fact (?) that the Indian words given in these accounts are not Indian at all, and (2) the introduction from Africa during the early sixteenth century or before, either by Europeans or Negroes, of yams, sweet potatoes, manioc and peanuts, together with tobacco and the practice of smoking—all of these having hitherto been generally regarded as of native American origin, or at least of long use in America prior to the Discovery.

The argument for the first of these theses is ingenious and in many respects plausible. Columbus in sailing westward "never for a moment doubted" that "he would reach China, Japan and the islands of the Indian Ocean, and he carried with him mappamondos in which that part of the world was given in the extreme East." Therefore, when he arrived at the Antilles he attempted to identify the islands which he saw with those shown on his maps, and the "Indian" names which he gave for these lands were merely misreadings and misspellings of the names on the Catalan, Fra Mauro and de Virga maps. Some of these suggested origins are, if satisfactory native etymologies are really lacking, very plausible; others are much less so, and some in spite of their ingenious character do not carry conviction and indeed in some cases rest upon



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misstatements. Etymology is a fascinating but precarious pastime, and if we select one of a series of variants, assume a certain amount of misspelling or misreading together with phonetic equivalences for which there is no proof, we may achieve almost anything!

In one at least of his attempts to prove the "atrocious forgery" of much of Columbus' writings and those of Ramon Pane, Professor Wiener shows a readiness to seek for and accept far-fetched explanations, a tendency which becomes more noticeable in his later chapters. Thus he rejects as a lie the story told by Columbus of fishing by the aid of the remora or sucking-fish, and declares it to have been derived from Odoric of Pordenone's account of cormorant fishing in eastern China. A little investigation would have shown that improbable as it may seem, there is no good reason to brand it as a pure invention or plagiarism, for precisely this same method has been and still is employed in Melanesia and its practicability has recently been demonstrated by tests in New York!

In his third chapter, the author deals at length with the question of tobacco and the custom of smoking, attempting to prove that tobacco was unknown in the New World until the plant and its name and use were introduced primarily by the Negroes, who were brought over as slaves during the first half of the sixteenth century. There are few things which have been regarded as more typically American than tobacco and its use, and one must admire the courage of the author in declaring this generally accepted belief to be wholly wrong. But a careful reading of the chapter in question leads only to amazement that anyone could, without the slightest regard for the facts of American archaeology and ethnology (with whose results in the last generation Professor Wiener appears to be wholly unacquainted) put forward so revolutionary a theory. The main steps in the argument seem to be (1) that tobacco and the practice of smoking were known to the West African Negroes prior to the end of the fifteenth century; (2) that the earlier explorers of the New World nowhere found tobacco in use, (3) that the words for tobacco, pipe, etc., in American Indian languages are in the main derived from the Mande words for the same, which go back ultimately to Arabic originals, and (4) that since all pipes must thus in the New World be post-Columbian, all archaeological remains with which they are associated are also post-Columbian.

As the earliest certain record the author has been able to discover referring to the use of tobacco in Africa is at the end of the sixteenth century, it is obviously incumbent on him, if he is to prove his theory, to find indirect evidence of its earlier presence. He does so in the names

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now used for tobacco by the Sudanese and Mande tribes, which names he attempts to derive from the Arabic "tubbaq" an aromatic plant whose leaves were used in Arabia for dressing wounds. Now tobacco being as he says (although without any satisfactory or valid evidence[1]) native in Africa, "took the place of the plants which were exotics" (*i.e.*, the original "tubbaq" of Arabia), and since "the magic Mussulman pharmacopoea" utilized "aromatic plants for burning" (*i.e.*, incensing) "there arose in Africa the habit of smoking." As it will be found on examination that nearly every stage in this "argument" rests on unverified assumptions, it is to say the least, hardly convincing. But this is the whole case for the African use of tobacco in smoking prior to the discovery of America!

The second stage in the argument, *viz.*, that we have no early accounts of tobacco or the use of smoking in America is equally unconvincing. He points out what is indeed a puzzling fact, that Columbus in his first voyage makes but one very uncertain reference to smoking, and that in the earlier accounts of Florida its use is not mentioned. On the other hand he minimizes and quite misunderstands (as well as mistranslates!) the evidence afforded by Sahagun and Bernal Diaz. He ridicules Oviedo's earlier errors in confusing the Antillean custom of inhaling cohoba (*Piptadenia peregrina*) with the smoking of tobacco, and denies *in toto* the former practice with its use of the bifurcated snuffing tube; a denial which, in view of Uhle's and Safford's careful studies, is without force. He also points out that the first description of smoking in Brazil dates only from 1555, and that Thevet then states that tobacco is called "petun." This affords an opportunity for one of the pieces of pure speculation in the philological field with which the volume abounds. For this word, widely diffused in the languages of the Tupi-Guarani stock, is, says Professor Wiener, derived from the Portuguese "betume" in turn derived from "bitumen." As the Arabic "tubbaq" was originally used to refer to the glutinous qualities of the leaves of the plant used in Arabia for dressing wounds, and later was transferred to tobacco which the Negroes learned to smoke, and since Arabic influence in medicine was not yet extinct in Portugal at the end of the fifteenth century, *therefore* "betume" since it also referred to a viscous substance "*must*" have acquired the same various meanings (*i.e.*, tobacco) as "tubbaq,"

[1] Of the two references given to prove that tobacco is native in Africa, one does not even refer to the subject, while the other clearly indicates the exact opposite to what Prof. Wiener says. Similar examples of gross carelessness or direct misrepresentation abound.

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and it was this word which, with the plant itself, had been taken by the Portuguese pilot of Pigafetta to Brazil more than thirty-five years before! Comment seems superfluous.

It is in connection with Cartier's account of smoking among the St. Lawrence tribes, however, that the author achieves still more amazing results. Being unable to deny Cartier's definite description and as no prior Negro influence here could, even by Professor Wiener, be assumed, it is necessary for him to show that the practice among the Iroquoian tribes at Montreal and Quebec was of recent introduction. His "evidence" for this rests in part on direct misquotations and misunderstandings, and in part on a failure to comprehend the character of Indian life and the conditions prevailing in eastern North America in the early sixteenth century; and includes a most absurd attempt at derivation. Professor Wiener states (p. 137) that Cartier "mentions figs, cloves and cinnamon, oranges, almonds and apples as known to the Indians and possessing Indian names." Later (p. 144) he adds "prunes" to this list. He first suggests (pp. 137, 145) that the Hurons knew of these tropical products through contact with the Breton fishermen who had preceded Cartier on the Canadian coast. Later (p. 146) he adopts the theory that the Hurons "were before the middle of the sixteenth century in some relation, apparently commercial, with Europeans on the Gulf of Mexico," and there obtained not only "oranges, cinnamon and cloves" but also tobacco for the first time. Further proof of this extraordinary theory is found in the supposed derivation of the Micmac, Abnaki and Natick words for tobacco from the Mande Negro "taba," while the Huron term is declared to be derived from the Arawak and Carib "iouli," which in turn goes back to a Mandingo form "duli" (pp. 184–5).

One is tempted to apply to this the term "balderdash" which the author uses in speaking of Columbus' writings, and while the whole is hardly worthy of serious comment, yet as it is typical of much of the author's whole method, it may be worth while to discuss it briefly. Professor Wiener in the first place directly misquotes his sources. Cartier does *not* say that the Indians had names for all of the seven articles which he enumerates, and he gives the names only for four of them, viz., figs, plums ("prunes" in English is not the equivalent of "prunes" in French!) cloves and cinnamon. The use of "apples" in Cartier's text is, as Professor Wiener failed to note, due to a misprint of "pommes" for "prunes." In the second place, the author accepts without the slightest apparent investigation, the identification of oranges, figs, etc., with Old World fruits. What, however, may reasonably be inferred

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from Cartier's statements? Obviously that the Indians, questioned in a language which they did not understand in regard to the products of their country, or asked for names of Old World products, gave in reply the names of native fruits which the French understood to be "oranges," "figs," etc., or which resembled the Old World products shown. This is a common experience and examples may be found in many parts of the world. Plums of course were native, the "oranges" may well have been the Osage orange or some variety of *Crataegus*, and "almonds" could have been any one of several varieties of nuts. In regard to "figs," it must be remembered that Cartier himself confused them with plums, and gives the same name for both. They may, however, be a reference to the "May Apple," called locally, "Indian Fig," or perhaps to a variety of *Opuntia*. There remain the "cloves" and "cinnamon." In regard to the former, it is to be noted that they are referred to in two of the three manuscripts as "so-called cloves," obviously indicating that they resembled but were not true cloves. It has been suggested that sassafras may have been what was meant by the word for "cinnamon." Under any circumstances, however, whether these suggested identifications are correct or not, there is a very weak place in Professor Wiener's whole argument. For, if tobacco, with its name, was imported to Ontario from the Gulf Coast, why do we not also find the names for these imported tropical fruits and spices *also* derived from Old World sources? What is sauce for the goose is sauce for the gander!

The theory that the Iroquoian tribes north of the St. Lawrence were, in the early sixteenth century, in direct trade relations with the Gulf of Mexico could, quite apart from its inherent improbability on account of the distances involved, only be imagined by one quite unaware of the character of Indian trade and of the political conditions among the eastern tribes at this time. One of the "proofs" of this supposed trade which is adduced shows to what lengths the author is willing to go in the way of, shall we say "fanciful," etymologies. On page 145ff. it is contended that the carriers of this trade in tobacco and tropical fruits were the Algonkian people called by Sagard (and by him alone!) the Epicerinys, whose name is derived by Professor Wiener with all apparent seriousness from the French "épicerie" (spices). It is hardly necessary to point out that these "bringers of spices" are the Nipissirini or Nipissings of the lake of that name in northern Ontario. Quite apart from its validity on the philological side, it may be wondered why the presence of assumed Mande words for tobacco among the Algonkian tribes of the Maritime Provinces and northeastern New England should be adduced as evidence for an overland trade by the Hurons with the Gulf of Mexico!

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Space is lacking to point out all the vagaries which fill the pages of this extraordinary chapter. These range from misstatements, such as when (p. 189) it is said that Alarcon in 1540 described Indians of the Northwest (sic) as "addicted to smoking, carrying the tobacco and the pipe in a bag tied to their arms," to the quite incomprehensible attempt to make the Mexican "chapopotli" (which was by the author's own statements, a bituminous, reddish-purple, aromatic material mixed with other substances in the filling of cigarettes) equivalent to meerschaum (pp. 149, 181–184); from the credulity which accepts without question Squier's identification of "manatee" and "toucan" pipes in the Ohio mounds (p. 168) to the assurance which, in utter disregard of all archaeological data, declares the pottery heads of San Juan Teotihuacan "negroid" and hence post-Columbian (p. 174); from the theory that the face tatu pattern shown on Arkansas pottery vessels is a direct copy of Mande cicatrices (p. 174) to the "amazing similarity" of African and North American Indian pipes. Professor Wiener's climax, however, is reached at the close of the chapter (pp. 189–190) in his discussion of the mounds, where he declares that "the very last vestige of a pre-Columbian existence of the mounds disappears" and asserts that all of the mounds were "fortifications which the traders, whether Whites or Indians, erected all the way up from Florida to the Huron country, in order to vouchsafe the trade which was established in the beginning of the sixteenth century . . . between Canada and the south." Before so simple, so comprehensive, so grandiose a conception as this, one can only stand in awe!

Much of the chapter on Bread Roots is vitiated by the same faulty reasoning and acceptance of unverified assumptions as facts, the same misrepresentations and contradictions, the same total neglect of important historical and all archaeological data. He shows, and shows clearly, that there is much confusion in regard to the yam, sweet-potato, manioc and peanut and their names in the accounts of the writers of the early sixteenth century, and that many of the names apparently have a distribution far beyond linguistic stock lines. He brings considerable evidence to show that some of these names may have been of Old World origin; but all of this does not entitle him to insist that the plants themselves were also foreign! His inconsistency here is very apparent, for while in the case of the words for monkey, he admits and indeed tries to prove "the rapidity with which foreign words were adopted by the natives even for native commodities, if these formed a subject of commerce" (p. 206), yet in the parallel case of manioc (whose commercial use he is

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at pains to emphasize, p. 214) this possibility is spurned, and because the name for manioc may be of foreign origin, the plant itself *must* be also. Contradiction has no terrors for the author, so that we find (pp. 238–239) that "there cannot be the slightest doubt" but that the sweet-potato was introduced into Asia and the East Indies from the Congo by the Spanish and Portuguese voyagers of the sixteenth century, while later (p. 261) he says that "it can be proved, beyond any possibility of cavil, that the sweet-potato was cultivated in Asia before the discovery of America." That the yam and sweet-potato were both widely cultivated in the Polynesian area prior to the first appearance of Europeans in the Pacific is not mentioned in Professor Wiener's whole argument, and the fact that actual specimens of sweet-potatoes and peanuts are found in prehistoric Peruvian tombs and are represented, together with manioc apparently, on Peruvian pottery of similar age seems quite unknown to the learned author, who triumphantly proves their non-existence on linguistic grounds!

Space is lacking to discuss adequately the purely philological portions of the volume. In general it may be said that the author depends in the main solely on similarities in sound, and quite disregards all questions of phonetic laws or the principles of word composition—a method whose great untrustworthiness linguistic students have long recognized. One or two examples of the author's methods will suffice to show the quality of his scholarship. Referring (p. 143) to the terms *uppowoc*, *uhpooc*, *apooke* used for tobacco in Virginia, he says: "One need only look at the juxtaposition of *tobacco-apooke* to convince oneself that the second is an apocopation of the first, the *t* appearing as a pronominal suffix" (sic). Further explanation of this pronominal "suffix" would doubtless interest students of Algonkian languages, but quite apart from this, the whole statement, if it means anything, would seem to imply that the word *tobacco* was derived from this Virginia Indian word *apooke*! Elsewhere it is always *taba*, *tawa* from which the American Indian words are supposed to be derived, and not from *tobacco*! Again, as proof of the Negro origin of Indian words used by Ramon Pane, Professor Wiener cites (p. 160) the Indian word *cobo* "a sea snail" and correlates it with the Malinke *kobo* "nom d'un insecte coléoptère"! To other minds the association is hardly obvious!

It is neither necessary nor profitable to bring forward further criticism. To point out all the errors of fact and reasoning, correct all the misunderstandings, misrepresentations and mistranslations, and refute the conclusions would require a volume in itself. Professor Wiener has

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rendered students of aboriginal American culture a distinct service in showing that there is much confusion in the accounts of the early writers, that a wide diffusion of certain plant names seems to have occurred during the century after the Discovery, and that African influence may have been something of a factor in it all. That, however, the plants themselves were of foreign origin and were unknown in America until introduced in the early sixteenth century, he has, in the reviewer's opinion, quite failed to prove. It is clear that there is a problem here which demands a scientific and scholarly study, but this the volume under discussion cannot be said to supply.

R. B. DIXON

*Vertebrate Zoölogy.* HORATIO HACKETT NEWMAN, Professor of Zoölogy and Embryology in the University of Chicago. The MacMillan Company: New York, 1920. Pp. xiii + 432, 217 figs.

A textbook of vertebrate zoölogy or comparative anatomy is necessarily largely a compilation. Its value is determined by the author's choice of material and authorities as well as his presentation of the subject. Professor Newman has been successful in a wise selection of material from the best books on this subject. The greater part of the data is derived from such authorities as Brehm, Cope, Flower, Lydekker, Gadow, Gegenbauer, Gregory, Haswell, Hertwig, Huxley, Jordan, Keibel, Kingsley, Lillie, Lull, Mall, Mathew, Minot, Osborn, Parker, Patten, Scott, Weber, Wiedersheim, Wilder, and Williston. This list is sufficient to show that the book contains well-balanced proportions of embryology, paleontology, comparative anatomy, and phylogeny. The first three subjects are used to good advantage in establishing phylogenetic relationships and tiresome details that do not serve this end are omitted.

A feature of the book is the introduction and application of Child's axial gradient conception in the interpretation of vertebrate structures. There are three axes of the vertebrate body: a primary antero-posterior axis, a secondary dorso-ventral axis, and a tertiary bilateral axis. The generalization is that the organs of highest dynamic activity are at the apical ends and those of least dynamic activity are at the basal ends of these axes.

The book is adequately illustrated with 217 text-figures. Although not one of these figures is original, the author's contribution here has been an important one. He has regrouped and combined figures from other authors to very good advantage.

Four pages are devoted to man. They are totally inadequate and

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from an anthropological point of view had better been omitted. The Australians are described as having woolly hair. The habitat of the Negroid race is given as "Madagascar and Africa from the Sahara Desert to the Cape of Good Hope," thus leaving out of consideration altogether the Melanesian Negroes and Negritos. The illustrations for this section are not particularly well chosen.

LOUIS R. SULLIVAN

*The Intellectuals and the Wage Workers. A Study in Educational Psychoanalysis.* HERBERT ELLSWORTH CORY. New York: The Sunwise Turn, 1919. 273 pp.

Only a small portion of this book has a direct bearing on topics of professional interest to anthropologists. Nevertheless, it is important as evidence of the ever widening influence of our science. When Professor Cory has occasion to seek enlightenment on matters of racial endowment and class psychology, he not only wisely comes for guidance to anthropologists but still more wisely gets his orientation from the foremost champion of scientific method in the field, Professor Boas. His rescuing one of Professor Boas' fugitive articles on caste is especially commendable. Another point that must impress the ethnologist favorably is the sanely broad conception of religion set forth in Chapter III, where incidentally application is made of some of Mr. Marett's ideas. The critique of Comte's triple-stage theory (p. 51) reveals sound sociological insight. Altogether Professor Cory has completely freed himself from the incubus of the unilinear evolution dogma. It is most gratifying to find so ready a response to our teachings on the part of a student of literature and psychoanalysis. Surely the greatest service we can do to the public at large lies in the dissemination of valid anthropological principles and the elimination of the solemn nonsense that often parades as scientific knowledge.

ROBERT H. LOWIE

## NORTH AMERICA

*Certain Aboriginal Pottery from Southern California.* GEORGE G. HEYE. (Indian Notes and Monographs, vol. VII, no. 1.) Museum of the American Indian, Heye Foundation, New York, 1919.

Mr. Heye describes in this paper over 400 pieces of pottery buried or hidden by the Diegueño and Luiseño Indians of former days and recovered by their descendants or by Mr. Edward H. Davis of Mesa Grande. Part of the collection had been used for mortuary purposes and con-

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tained ashes and calcined human bones. The type has been known and a few sporadic specimens have been described, but the size of the present collection definitely establishes certain points: notably that the mortuary vessel, instead of being specially made, was an ordinary water or seed storage jar; also, that it was more frequently buried in the cremation pit than carried to a cave or recess. The account of funeral customs given by a Diegueño, as related on pp. 13–19, corroborates the data of DuBois and others on the religious aspects of death and adds several new features, such as the breaking of the burned bones by an old female relative.

The artifact contents of the mortuary vessels described on pp. 36–45 suggest that the Luiseño-Diegueño material culture of early Mission and pre-Caucasian days was not notably richer than the collections and memories of recent years indicate: arts were few and scantily advanced.

As to the pottery of the region, it is becoming more and more clear that this is an almost exact replica, except for some technical and aesthetic inferiority, of that made by the Yuman tribes of the Colorado. Their pottery, in turn, is not an offshoot of ancient or modern Pueblo ware, but very closely linked with that of the Pima and Papago—not so much in the modified present condition of the latter but as it was made before Caucasian influences began. This prehistoric ware of the Papago region may have affiliations with Sonora; if it traces back to the Pueblos, the transitions remain to be pointed out. It is interesting that of the large series of vessels on which Mr. Heye's report is based, barely two percent are painted. Among the Yuma and Mohave the majority of pieces are figured. Luiseño-Diegueño ware is therefore a crude provincial and peripheral imitation of the Yuman pottery, which itself is none too eminent for quality.

The author's finding is that the ceramic art among the Diegueño is not an ancient one (p. 22). This conclusion seems warranted; with the reservation that the industry is nevertheless pre-Caucasian, possibly by a number of centuries. A site on the southern edge of a lagoon a mile north of La Jolla is strewn with sherds. Examinations made there by Mrs. S. K. Lothrop proved all the pottery to come from near the surface; although the whole deposit was rather shallow. Nowhere in southern California have there been any accredited reports of potsherds being found at deep levels. This is one of the few matters in which close observation of stratification promises to be a fruitful method of attack in California archaeology.

At the risk of appearing to carp, it may be mentioned that a Diegueño mortuary olla was mentioned and figured by Waterman (Univ. Calif.

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Publ. A. A. E., vol. VIII, p. 306, pl. 23, 1910); and that Boscana's Indians were not strictly Luiseño but Juaneño and in part Gabrielino.

The material preserved and discussed in this little monograph by Mr. Heye is a valuable series; the precision and compactness of his descriptions, and the sanity of his findings, are pleasing.

A. L. KROEBER

*Calendars of the Indians North of Mexico.* LEONA COPE. (University of California Publications in American Archaeology and Ethnology, vol. XVI, no. 4, November 6, 1919, pp. 119–176, 3 maps.)

This paper is the product of a remarkable pedagogical device originated by Professors Kroeber and Waterman in their joint management of a proseminar at Berkeley. Confronted with a group of students often intelligent and eager to work but handicapped by a relatively slight degree of scientific training, they have selected and assigned problems—usually of distribution—that called for solution yet were not disproportionately difficult for the participants.

Miss Cope's essay is the first publication that developed from this course and amply justifies the method of procedure. With great industry she has gone over the available literature and abstracted relevant data. Her search has resulted in the tentative establishment of three types of calendars: descriptive, astronomical, and numeral. The first is characterized by the exclusive use of descriptive designations for the lunar months; it is spread over the Mackenzie, Northeastern and Southeastern Woodland areas, and occurs among some of the Southwestern nomads. In the Northwest and Southwest, as well as among some of the Eskimo, a recognition of the solstices is linked with descriptive terms. Finally, there is the numbered type in which numeral designations partly or entirely supplement descriptive terms; its distribution is restricted to the Northwest and adjoining regions. As Miss Cope takes pains to point out, there is throughout a clear predominance of the descriptive nomenclature, astronomical and numeral motives playing a subsidiary part. From the point of view of distribution, of course, minor features become significant and Miss Cope has carefully plotted some of them on her maps. She merits the gratitude of ethnologists for having so faithfully achieved an arduous task and facilitated the labors of future workers in this field.

ROBERT H. LOWIE

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## EUROPE

*Eine völkerkundliche Sammlung von den europäischen Samojeden.*. A. JACOBI. (Abhandlungen und Berichte des Königl. Zoologischen und Anthropologisch-Ethnographischen Museums zu Dresden, Band XV, 1917, Nr. 2.) Leipzig: B. G. Teubner, 1917. 4°, 17 pp., 4 pls., 7 figs.

In 1913 Dr. Jacobi undertook a zoological expedition to the Kanin peninsula for the purpose of pursuing studies on the reindeer and incidentally made a collection of Samoyed ethnographica. Later the collection was enlarged by additional pieces from the Timan and the Great Tundra, so that it may be taken as representative of the material culture of the European Samoyed. Dr. Jacobi briefly describes the principal objects secured for the Dresden Museum and reproduces many of them on his plates. In addition, while disclaiming the title of ethnographer, he records his observations on a number of points that must be of interest to anthropologists.

The author characterizes the culture of the Samoyed as having always been meager in comparison with that of other Arctic peoples. Naturally in course of time there have been numerous intrusions of Caucasian civilization, and among them the curse of alcoholism. Jacobi found that wealthy reindeer-breeders were impossible people for his purposes, since they had either substituted articles of Russian manufacture for those of aboriginal workmanship or disdained to take a few rubles for articles even if no longer of use to them. Families impoverished by the loss of their herds proved much more satisfactory, having generally retained old specimens for lack of means to supplant them with their store equivalents.

Of course Dr. Jacobi's publication does not purport to be a monograph and must be appraised for its positive qualities rather than for omissions that would call for comment in a more pretentious paper. His concise summaries of certain phases of Samoyed culture will be welcome to those who, like the present writer, approach the publication not as specialists but merely as general ethnologists. From this angle his comment on the superiority of Samoyed skin-dressing over that of the Lapp is interesting. Recent times have produced a considerable alteration in the process of chamoising: a layer of flour (*eine Mehlschicht*) induces fermentation, while formerly brains, liver, and the yolk of birds' eggs were used. Tobacco was mainly snuffed; pipes were not used at all in Kanin and only to a very limited extent in the Great Tundra. The only type of snowshoe current among the Samoyed is the ski, which has been adopted by Russian peasants and by urban hunters as well.

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Dolls form the most popular of children's toys; the head consists invariably of a wild goose's or duck's bill.

The excellent illustrations add materially to the value of this paper for museum curators.

ROBERT H. LOWIE

## SOME NEW PUBLICATIONS

Aranzadi, Telesforo de. Los Gentiles del Aralar. Bilbao: Juan J. Rochelt, 1919. 39 pp., 1 map, 12 figs.

——. El Tipo y Raza de los Vascos. Bilbao: Juan J. Rochelt, 1919. 30 pp., 3 maps, 9 figs.

——. Cráneos de Guipuzcoa. (Asociacion Española para el Progreso de las Ciencias: Congreso de Madrid.) Madrid: Eduardo Arias, 1913. Pp. 1–55.

——, and Ansoleaga, T. de. Exploracion de catorce Dolmenes del Aralar. Pamplona: Imprenta provincial a cargo de M. Falces, 1918. 53 pp., 1 map, 13 figs., 30 pls.

Beaver, Wilfred N. Unexplored New Guinea. With an Introduction by A. C. Haddon. Philadelphia: J. B. Lippincott, 1920. 320 pp., 32 ills., 4 maps.

Bushnell, David I., Jr. Native Villages and Village Sites East of the Mississippi. (Bureau of American Ethnology, Bulletin 69.) Washington, 1919. 111 pp., 17 pls., 12 figs.

Cooke, George Willis. The Social Evolution of Religion. Boston: The Stratford Company, 1920. xxiv + 416 pp.

Gaupp, Ernst. August Weismann, sein Leben und sein Werk. Jena: Gustav Fischer, 1917. viii + 297 pp.

Ischer, Th. Die Chronologie des Neolithikums der Pfahlbauten der Schweiz. (Sonder-Abdruck aus dem Anzeiger für schweizerische Altertumskunde 1919.) Ernst Bircher: Bern, 1920. 28 pp., 23 figs.

Jijon y Caamaño, J. La Religion del Imperio de los Incas. I: Los Fundamentos del Culto. Huacas, Conopas, Apachitas, Urcos, Huancas, Machais. Quito: Tipografia y Encuadernacion Salesianas, 1919. IV, 452 pp.

Keith, Arthur. The Differentiation of Mankind into Racial Types. (Address to the Anthropological Section, British Association for the Advancement of Science, 1919.) 7 pp.

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Kroeber, A. L. Peoples of the Philippines. (American Museum of Natural History, Handbook Series No. 3.) New York, 1919. 224 pp., 6 maps, 42 figs. and frontispiece.

Lang, Herbert. Nomad Dwarfs and Civilization. (Natural History, vol. XIX, 1919, pp. 696–713.)

Laufer, Berthold. Ethnographische Sagen der Chinesen. (Aufsätze zur Kultur- und Sprachgeschichte, vornehmlich des Orients, Ernst Kuhn zum 70. Geburtstage 7. II. 1916 gewidmet, pp. 198–210.)

Lowie, Robert H. Primitive Society. Boni and Liveright: New York, 1920. viii, 463 pp.

Morgan, Thomas Hunt. The Physical Basis of Heredity. Philadelphia and London: J. B. Lippincott Co., 1919. 305 pp., 117 figs.

Pearson, Karl and Bell, Julia. A Study of the Long Bones of the English Skeleton. (Department of Applied Statistics, University of London, University College. Drapers' Company Research Memoirs. Biometric Series X.) Text. Part I. The Femur, Chaps. I to VI. VIII, 224 pp. Atlas, Part I. 59 pls., 13 sheets. Cambridge University Press, 1919, 1917.

Routledge, Mrs. Scoresby. The Mystery of Easter Island. The Story of an Expedition. London: Sifton Praed and Co., Ltd., 1920. 9 × 6 in., xxi + 404 pp., 11 maps, 119 pls., 17 figs.

Schmidt, P. W. Die Gliederung der Australischen Sprachen. Geographische, bibliographische, linguistische Grundzüge der Erforschung der australischen Sprachen. Vienna: Mechitharisten-Buchdruckerei, 1919. xvi + 299 pp., 1 colored map.

Sullivan, Louis R. The Pygmy Races of Man. (Natural History, vol. XIX, 1919, pp. 686–695.)

Trabue, M. R. The Intelligence of Negro Recruits. (Natural History, vol. XIX, 1919, pp. 680–685.)



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# DISCUSSION AND CORRESPONDENCE

## The Reindeer Once More

THERE is as yet no exhaustive or real history of any animal domestication or plant cultivation, and such a task will still be impossible for a long time to come. Naturalists, biologists, geographers, historians, ethnographers, and orientalists, have made numerous contributions to these subjects, every one from the particular angle of his field; and, as is well known, their results are widely divergent and cannot yet be harmonized. Whoever has had occasion to work on these problems feels only too well that he is merely able to make a contribution to a problem, and makes no pretense of solving the problem in its entire complexity. Dr. G. Hatt has recently published an article on Reindeer Nomadism (*Memoirs, American Anthropological Association*, vol. VI, no. 2), which is partially devoted to a criticism of my former contribution to these Memoirs on the same subject. Dr. Hatt's paper doubtless contains many interesting references and notes, especially as far as his own field, the Lapp, is concerned; but I find it necessary to point out a number of misunderstandings and to discuss briefly some of his conclusions which are unacceptable to me.

Dr. Hatt claims that my disregard of the biology of the reindeer "seriously impairs the value of my theories about the origin of reindeer-domestication." This criticism is hardly fair, for I have not given any theories in regard to such origins, nor do I believe that in general origins can be explained satisfactorily. I hold that facts mean everything and that theories are of no account, and have plainly enough indicated (p. 129 of my article) that we are ignorant of how the initial domestication of the reindeer was brought into effect. I have then arrayed a number of available data which might give us a clew as to how this process came about, leaving it to whoever so desired to reconstruct this process according to his own liking. I did not attempt "to trace the evolutionary history of reindeer nomadism," as Dr. Hatt wishes me to do; for like Boas, Lowie, and others, I have always opposed the evolutionary method in its application to anthropological problems (cf. this journal, 1917, p. 299, with reference to Dr. Hatt's theory of the evolution of moccasins).

The essential points discussed by Hatt are all contained in my notice



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of the reindeer. It seems to me that the points made in this paper, regarding the relative age of the cultural elements of reindeer nomadism, can hardly be maintained. The criteria made out for earlier and later phenomena are purely subjective and a matter of debatable opinion. The vagueness of his chronology is not helpful in historical investigation. History must be based on historical data and documentary evidence, not on speculation. The account of Rubruck of the thirteenth century, Dr. Hatt quotes as proving the early use of ox or horse carts by the nomads, is of little value in view of the ancient accounts of the carts used by the nomadic Scythians in Hippocrates (*cf.* Minns, *Scythians and Greeks*, p. 50) and by the Turkish tribes in the Chinese Annals many centuries before that time. I fully maintain my point that the domestication of the reindeer presents a secondary and imitative process leaning toward horse and cattle, and as regards driving, toward the dog. Hatt denies the former influence, but then he hastens to explain that "reindeer milking certainly must be due to influence from cow or horse culture," and again, "the use of the reindeer for riding and carrying and as a milk-giving animal must have come into the Tungusian-Soyotian area as a result of contact with horse and cow culture;" and finally, "it is not to be denied that some reindeer nomads have taken over certain things from horse and cattle breeding."

I do not see that the data relating to the milking of reindeer which Dr. Hatt quotes alter the views expressed by me. Pekarski states plainly in regard to the Tungus of Ayan that butter is made to a small extent only, and this isolated case of modern origin is an exception which confirms the rule that butter was formerly not made by the Tungus in general. The fact that the Soyot consume reindeer milk in the shape of butter or cheese was stated by myself (p. 127). The chapter "Beginning of Reindeer Nomadism" is based on unproved premises and hypothetical and arbitrary speculations. Olsen (p. 113), according to Hatt, is wrong in his observations among the Soyot and must have misunderstood what he saw, because it so happens that his data contradict a theory of Hatt. This procedure seems to me entirely inadmissible, because it is based on the desire of the speculative theorist who combats the facts which disturb or shatter his dreams.

I strictly maintain my interpretation of Ohthere's account. Nowhere have I entertained any doubt as to the nationality of the Finn, as Hatt supposes. His two objections to my interpretation (p. 120) are not valid. How do we know that a Norseman in the ninth century would never think of keeping deer in a park? The supposition that "the reindeer is a

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migratory animal which cannot be kept in parks or enclosures" is unproved. For at least twelve years I have observed a couple of reindeer in a zoological garden, and they were perfectly happy and content there. The reindeer of the Soyot is not at all migratory, but during the summer the herds constantly remain in the forest in the proximity of human habitations (after Olsen, in my article, p. 127). When in the summer of 1898 I resided in the settlement Wal among the Ewunki Tungus on the northeast coast of Saghalin island, the reindeer herds of this tribe were kept in confinement on a small isle hardly two miles square, which they were unable to leave; they were held there in a perfect enclosure formed by water. Any park or enclosure may certainly be large enough to allow an animal to yield to its migratory habit. Giles Fletcher (*Of the Russe Common Wealth*, London, 1591, ed. of E. A. Bond, 1856, p. 101), in his description of the life of the Lapp, states, "Their travaile to and fro is upon sleds, drawen by the Olen deer; which they use to turne a grasing all the sommer time in an iland called Kilden (of a very good soile compared with other partes of that countrie), and towards the winter time, when the snow beginneth to fall, they fetch them home again for the use of their sledde."

We do not read more from or into our documents than is warranted by their contents, and Ohthere does not say a word about the Lapp tending his herds. There is as yet no proof for the allegation that the Lapp of the ninth century were reindeer nomads. Frijs says advisedly, "The Lapp in the north of Scandinavia during the ninth century were still fishermen and hunters, and were only acquainted with reindeer as game, while they did not yet possess tame animals" (C. Keller, *Naturgeschichte der Haustiere*, p. 200). Dr. Hatt objects to this statement that our forefathers "were not ethnographers"; but this is no argument. The interpretation of reindeer into the harts put to the cart of Hotherus (p. 125) is not safe: the tale of Saxo is legendary, not historical. Also the Romans and Chinese harnessed stags to carriages (see my article, pp. 132, 133), and no one would think of claiming that these were tamed reindeer. In the opinion of the best philologists of our time, particularly those of France, no historical facts should be deduced from the status of loan-words and other linguistic phenomena (against Hatt, p. 128); if this is done, however, the conclusion will always remain an hypothesis, but will never rise into a fact. That Hatt, after offering not a single piece of tangible evidence, should advance the assertion, "That reindeer nomadism existed in Scandinavia in the ninth century, and even somewhat earlier, may accordingly be regarded not as a mere hypothesis,

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but as a solid fact," is beyond my comprehension. I apprehend that a deep gulf separates us as to what constitutes a solid fact. There is not even room here for an hypothesis.

Dr. Hatt's discussion of the Kalewala is based more on an attempt to sustain his theory than on objective evidence. I had occasion myself to read this work repeatedly and at different times, and considerable literature about it. The book of Comparetti on which I chiefly relied is justly regarded as a classic throughout the civilized world, and it may be expected that a man who devoted a lifelong and serious study to this vast and complex subject knows at least as much about it as Hatt. Naturally there is much controversial matter and divergence of opinion with respect to the Kalewala, in the same manner as in the case of the Homeric poems, the Rigveda, or the Avesta. Dr. Hatt passes off his own ideas as "the truth about Kalewala" (p. 127) and denies categorically that it represents a true and perfect picture of the Finn prior to their christianization. May it not be that his judgment is influenced by the fact that the Kalewala runs counter to his theories, for it does not contain the faintest allusion to domesticated reindeer, while the wild reindeer was an object of the hunt, while sledge-driving is most frequently mentioned, but the sledges are always drawn by horses (my article, p. 191).[1]

Little troubled Lemminkäinen,
And he spoke the words which follow:
"Make a snowshoe left to run with,
And a right one to put forward!
I must chase the elk on snowshoes,
In the distant field of Hiisi."
XIII, 59–64.

"Let the men who live in Lapland,
Help me all to bring the elk home;
And let all the Lapland women
Set to work to wash the kettles;
And let all the Lapland children
Hasten forth to gather splinters;
And let all the Lapland kettles
Help to cook the elk when captured." etc.
But the third time he rushed onward,
Then he reached the elk of Hiisi.
Then he took a pole of maple,
And he made a birchen collar;
Hiisi's elk he tethered with it,

[1] I select several passages (translation of W. F. Kirby) in support of my above statement. If all this is not realism of cultural conditions, the Kalewala assuredly is pretty well consistent in its madness.

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In a pen of oak he placed it.
"Stand thou there, O elk of Hiisi,
Here remain, O nimble reindeer!"
XIII, 203–210, 217–224.

Glide throughout the land of Hiisi,
And across the heaths of Pohja,
There to chase the elk of Hiisi,
And to catch the nimble reindeer.
XIV, 19–22.

Thereupon the colt he harnessed,
In the front she yoked the bay one,
And she placed old Väinämöinen
In the sledge behind the stallion.
VII, 349–352.

Väinämöinen, old and steadfast,
Took his horse of chestnut color,
And between the shafts he yoked him,
Yoked before the sledge the chestnut,
On the sledge himself he mounted,
And upon the seat he sat him.
X, 1–6.

Thus the smith, e'en Illmarinen,
Clothed himself, and made him ready,
Robed himself, and made him handsome,
And his servant he commanded:
"Yoke me now a rapid courser,
In the sledge adorned so finely,
That I start upon my journey,
And to Pohjola may travel."
Thereupon the servant answered,
"Horses six are in the stable,
Horses six, on oats that fatten;
Which among them shall I yoke you?"
XVIII, 379–390.

I cannot see what gives us the right to say that "To regard the descriptions of Lapland and the Lapp, contained in Kalewala, as realism, would be perfectly ridiculous (p. 127)." Had it so happened that the Kalewala furnishes the opposite data which would support Hatt's presumptions, he would probably have accepted them without hesitation.

While I have to disagree with Dr. Hatt on many points, and am compelled to reject his claims, there is one point, however, on which I am heartily in accord with him, and this is his plea for collecting more mate-

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rial, before we shall know all about reindeer nomadism. What we need are facts and research based on serious information. We live to learn and to work.

Finally I may be allowed to quote a passage from a letter of the late Dr. Herman K. Haeberlin, in memory of a friend who was always dear to me. On November 1, 1917, our regretted friend wrote me from Columbia University, New York, as follows: "I was very much interested in your paper on the reindeer. Aside from its value as the investigation of a concrete cultural trait, its methodology I think is highly instructive for us anthropologists. It shows what can be attained by a scholarly coöperation of direct historical reconstruction and indirect ethnological inference. Furthermore, an important methodological point is that you trace the origin of reindeer domestication to a definite geographical area rather than to a certain tribe. This methodological distinction ought to be borne in mind more clearly than we have thus far done. I shall attempt to make the same point when I discuss the center of distribution of imbricated basketry in North America."

B. LAUFER

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# ANTHROPOLOGICAL NOTES

DURING the March meeting of the Cayuga County Historical Society, Mrs. Mary Clark Thompson of Canandaigua was awarded the Cornplanter Medal for marking the sites of Iroquois villages and burial sites and for her endowment of the Iroquois section of the New York State Museum. This award, like that to William Pryor Letchworth, was for philanthropy rather than for direct contributions to knowledge. The 1918 medal was awarded to Alvin Hiram Dewey, President of the Lewis H. Morgan Chapter of the New York State Archeological Society for his work in organizing a state-wide association of archaeologists and for his success in stimulating numerous students to a scientific study of the New York aborigines.

MR. ARTHUR C. PARKER, archaeologist of the New York State Museum commenced operations on Boughton Hill, Ontario county, early in May. Boughton Hill is the site of the Seneca capital destroyed by Denonville in 1687. During the autumn of 1919, Mr. Parker discovered numerous graves in the site and made a considerable collection that included numerous specimens of wood and fabric preserved by contact with brass and copper objects.

DR. TRUMAN MICHELSON, of the Bureau of American Ethnology, has left Washington with the intention of continuing his investigations among the Fox Indians near Tama, Iowa. About the first of July he expects to begin a rather extended visit to various other Algonquian tribes which will occupy about three months.

THE League of the Six Nations of Canada is now engaged in litigation with the Dominion authorities whereby it seeks to retain its national identity. This "confederacy" of the Iroquois claims ancient origin and that it has always been recognized by British authorities as independent. Its claim is that in its relations to Canada it has served as an ally of Great Britain and not as a subject people. The Six Nations Confederacy claims to occupy a domain and not a reservation and further that it has an effective constitutional government that antedates that of the Dominion of Canada. This struggle of the descendents of the famous Iroquois League to retain its identity will be watched with interest.



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DURING January, The Mohawk Valley Chapter of the State Archaeological Association was instituted in Schenectady with Langdon Gibson as President and Dr. W. W. Whitney as Vice-President.

DR. ALEŠ HRDLIČKA was absent from Washington from the latter part of January until the middle of May on a visit to China, Japan, Korea, Mongolia, Manchuria, and Hawaii, where he engaged in scientific work in various localities and delivered a series of lectures before the Union Medical College, Peking, under detail from the Smithsonian Institution.

MR. GERARD FOWKE left St. Louis on April 1 for Honolulu, where he will make an archaeological reconnoissance of the Hawaiian Islands with a view to future intensive work by the Bureau of American Ethnology.

EARLY in May, Dr. J. W. Fewkes, Chief of the Bureau of American Ethnology, visited Nashville, Tenn., where he delivered an illustrated lecture on the "Cliff Dwellings of the Mesa Verde" at the Centennial Club before the Tennessee Historical Society, and the Nashville Branch of the Archaeological Institute. On the 19th he left Washington for a short period of archaeological work in the Mesa Verde National Park.

DR. WALTER HOUGH, of the U. S. National Museum, left Washington at the end of May for two months work in Arizona among the Hopi and Apache Indians.

DR. JOHN R. SWANTON, of the Bureau of American Ethnology, has been made a Corresponding Member of the Societe des Americanistes de Paris.

MR. J. A. JEANCON, of Colorado Springs, was engaged during March and April in working over the archaeological collections gathered by him last summer near Abiquiu, New Mexico, for Mr. Otto T. Mallery and in preparing a report on his observations for the Bureau of American Ethnology. The collections have been presented to the Bureau by Mr. Mallery and will later be transferred to the National Museum.

AT the request of the National Geographic Society, the Secretary of Smithsonian has granted permission for Neil M. Judd, Curator of American Archaeology, to direct the Society's archaeological reconnaissance of

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Chaco Canyon, New Mexico, and to collect material for the National Museum. Should the results of this preliminary expedition warrant, it is understood that the Society is prepared to undertake more intensive investigations in the future. This survey will follow certain researches in northwestern Arizona which Mr. Judd is to make for the Bureau of American Ethnology, concluding his examination of the archaeological remains north and west of the Rio Colorado. Mr. Judd left Washington on May 1st in the interest of this second detail.

PROFESSOR G. ELLIOT SMITH visited the American Museum of Natural History in May and had personal discussions on the diffusion of cultural elements and the independence of American culture with Drs. Wissler, Lowie, and Spinden.

IN February, Dr. W. H. R. Rivers, of Cambridge, England, visited this country under the auspices of the New York Psychiatrical Society in order to give a series of lectures on psychiatry and psychoanalysis in Baltimore, New York, and other centers of research along these lines. He also found time to join the American ethnologists of the East on several occasions and on March 15, under the auspices of the New York Academy of Sciences and the American Ethnological Society, he delivered an address on "Ethnology: its Aims and Needs." Dr. Rivers returned to England in April.

AT Munich Drs. Walter Lehmann, Leo Frobenius, and Weber have founded a research institute for ethnography (Forschungs-Institut für Völkerkunde).

DR. KARSTEN has returned to Sweden after a three and a half years' sojourn in Ecuador, devoted mainly to the study of the Colorado and Jibaro Indians. An essay on the mythology of the latter tribe appears in the *Boletin de la Sociedad ecuatoriana de Estudios historicos americanos*, 1919, no. 6.

Father Laurent Le Goff has received the Loubat prize for his *Dictionnaire français montagnais, précédé d'une explication de l'alphabet et d'un tableau des principales racines* (Paris, 1916).

AT a meeting of the Division of Anthropology and Psychology of the National Research Council held in Washington, April 17, Professor J. H.

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Breasted was chosen to serve three years as a member-at-large. Dr. Clark Wissler was elected Chairman of the Division. Dr. Wissler will be in Washington after September 1st.

DRS. WISSLER, Tozzer, and Kroeber, expect to attend, during August, the scientific conferences in Honolulu during which work among the Polynesians will be discussed and planned.

MR. RALPH LINTON and Mr. Edward S. Handy sailed in June to begin ethnological and archaeological work in the Marquesas islands.

MR. E. W. GIFFORD and Mr. McKern are also to undertake work in Polynesia. Mr. Gifford has been granted a leave of absence for the purpose by the University of California.

DR. LESLIE SPIER has been appointed Associate Curator of the Museum of the Department of Anthropology of the University of California. The appointment is a temporary one. Dr. Spier is to take up the work of Mr. Gifford during the latter's leave of absence.

AT the Commencement of Columbia University, June 2, three higher degrees were given in anthropology. Miss Gladys A. Reichard and Miss Erna C. Gunther received the degree of A.M., and Leslie Spier that of Ph.D. Dr. Spier's thesis is entitled "The Sun Dance of the Plains Indians." It will appear in the *Anthropological Papers of the American Museum of Natural History*.

MR. WM. CHURCHILL, the well-known student of Polynesian linguistics and ethnology, died in Washington, D. C., June 9, 1920. Mr. Churchill was born in Brooklyn, N. Y., October 5, 1859. He graduated from Yale University in 1882. Many years of his life were spent in the Pacific where he held consular appointments. Since 1915 he had been connected with Carnegie Institution.

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# INDIAN CORN-HILLS IN MASSACHUSETTS

BY EDMUND B. DELABARRE AND HARRIS H. WILDER

## I. HISTORICAL INTRODUCTION

THE fact that there are still in New England perfectly preserved and unmistakable remains of the small mounds or hills in which the Indians planted their maize and other crops, seems not to be generally recognized. Even for other parts of the country, reports of the existence of such "Indian garden beds" are very few. I. A. Lapham[1] tells of their occurrence in Wisconsin, and it is said that Cheney[2] found them in western New York. Sir John Lubbock[3] quotes Lapham. Lapham's description is as follows: In many places

> the ground is covered with small mamillary elevations, which are known as Indian corn-hills. They are without order of arrangement, being scattered over the surface with the utmost irregularity. That these hillocks were formed in the manner indicated by their name, is inferred from the present custom of the Indians. The corn is planted in the same spot each successive year, and the soil is gradually brought up to the size of a little hill by the annual additions.

In one of his localities,

> another evidence of former cultivation occurs, consisting of low, broad, parallel ridges, as if corn had been planted in drills. They average four feet in width, twenty-five of them having been counted in the space of a hundred feet; and the depth of the walk between them is about six inches. These appearances,

[1] *The Antiquities of Wisconsin*, 1855, pp. 19, 57; *Smithsonian Contributions to Knowledge*, vol. VII.

[2] On Ancient Monuments in Western New York; *Thirteenth Report of the Regents of the State of New York*, 1860, p. 40. The writers have not been able to verify this citation.

[3] *Prehistoric Times*, 1913 ed., p. 273.



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which are here denominated "ancient garden-beds," indicate an earlier and more perfect system of cultivation than that which now prevails.

In another place,

the depressions or walks between the beds were about eight inches deep and fifteen inches wide.

As to the existence of such remains in New England, we seem to have only a statement by Jeremy Belknap, applying to New Hampshire, and a doubtful report from Massachusetts. Belknap wrote in 1792:

The remains of their fields are still visible in many places; they were not extensive, and the hills which they made about their corn stalks were small.[1]

The Massachusetts instance is mentioned in the Handbook of American Indians North of Mexico, which does not seem to take it seriously. Under the heading "Indian-corn hills," it remarks: "In Essex Co., Mass., according to Bartlett, hummocky land resembling hills of Indian corn."[2] The original description by Bartlett we have not traced to its source.

Mr. William M. Cotton, of Providence, informs the writers that when he was a boy, living in Pomfret, Connecticut, in uncultivated pastures on his farm there were numerous small mounds that were generally known as Indian corn-hills. Whether they still exist and are so known, we have not learned. We have personally investigated, however, two localities in Massachusetts in which the ancient "Indian corn-hills" unquestionably persist.

That there should be any traces left at the present day of the gardens cultivated by the Indians two hundred and fifty years ago and earlier, may at first sight seem incredible. But there are many fields in New England, probably some in every town, that have always been used by white men as pastures, and have never been touched by the plough. Any of these which may have been used by the Indians as corn fields would stand an excellent chance of having the hills indefinitely preserved, because of the way in which the Indians did their planting. The early settlers and explorers give interesting accounts of this, and we may advantageously

[1] *History of New Hampshire*, vol. III, p. 88.

[2] Vol. I, 607.

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preface our own observations with quotations from them and others.

Although the cultivation of corn (maize) by the aborigines of the West Indies was observed and reported by the earliest of the discoverers, probably Samuel de Champlain was the first to give any account of this form of agriculture in New England. His first recorded observation was made during his voyage of the summer of 1605, at or near what is now Saco, Maine, his "Choüacoet." He writes:

The next day [July 9, 1605] Sieur de Monts and I landed to observe their tillage on the bank of the river [Saco river]. We saw their Indian corn, which they raise in gardens. Planting three or four kernels in one place, they then heap about it a quantity of earth with shells of the *signoc* before mentioned [the horse-shoe crab, *Limulus polyphemus*]. Then three feet distant they plant as much more, and thus in succession. With this corn they put in each hill three or four Brazilian beans [the kidney bean, *Phaseolus vulgaris*], which are of different colors. When they grow up, they interlace with the corn, which reaches to the height of from five to six feet; and they keep the ground very free from weeds. We saw there many squashes, and pumpkins, and tobacco,[1] which they likewise cultivate. The Indian corn which we saw was at that time about two feet high, some of it as high as three. The beans were beginning to flower, as also the pumpkins and squashes. They plant their corn in May, and gather it in September[2] (p. 62).

When in Boston Bay, about the mouth of the Charles, he records that

they brought also some purslane, which grows in large quantities among the Indian corn, and of which they make no more account than of weeds. We saw here a great many little houses, scattered over the fields where they plant their Indian corn (p. 67).

Reaching the harbor of Nauset on Cape Cod he says that they

went about a league along the coast. Before reaching their cabins, we entered a field planted with Indian corn, in the manner before described. The corn was in flower, and five and a half feet high. There was some less advanced, which

[1] "The tobacco of the New England Indians was *Nicotiana rustica*, not *N. tabacum*. The former is inferior, and now grows wild in old fields in some parts of the north, a relic of cultivation by the Indians."—*Mourt's Relation*, ed. Dexter, p. 36n.

[2] These quotations from Champlain are taken from the recent translation made by W. L. Grant (Oxford), which appeared in 1907, as one of the series of *Original Narratives*, edited by J. Franklin Jameson, and published by Scribner's. The pages here given correspond to this first edition.

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they plant later. We saw many Brazilian beans, and many squashes of various sizes, very good for eating; some tobacco, and roots which they cultivate, the latter having the taste of an artichoke.[1] . . . There were also several fields entirely uncultivated, the land being allowed to remain fallow. When they wish to plant it, they set fire to the weeds, and then work it over with their wooden spades (p. 71).

When the Indians wished to clear forest land they did it in great part by help of fire, according to information obtained in the harbor of Gloucester, Mass.

Some of the land was already cleared up, and they were constantly making clearings. Their mode of doing it is as follows: after cutting down the trees at the distance of three feet from the ground, they burn the branches upon the trunk, and then plant their corn between these stumps, in course of time tearing up also the roots (p. 92).

The corn, thus grown, the Indians were accustomed to store in granaries situated nearby and placed partly beneath the surface of the ground. These are locally known as "Indian barns," and have been frequently located in the Connecticut river towns in Massachusetts [*e.g.*, George Sheldon, in the History of Deerfield]. These Champlain describes as he finds them at Chatham, Mass., on the Cape.

There is a considerable quantity of land cleared up, and many little hills, where they cultivate corn and the various grains on which they live. . . . All the inhabitants of this place are very fond of agriculture, and provide themselves with Indian corn for the winter, which they store in the following manner: They make trenches in the sand on the slope of the hills, some five to six feet deep, more or

[1] *Helianthus tuberosus*, Jerusalem Artichoke. Its history is thus described by Neltje Blanchan, in Nature's Garden: "In a musty old tome printed in 1649, and entitled 'A Perfect Description of Virginia,' we read that the English planters had 'rootes of several kindes, Potatoes, Sparagus, Carrets and Hartichokes'—not the first mention of artichokes by Anglo-Americans. Long before their day the Indians, who taught them its uses, had cultivated it; and wherever we see the bright yellow flowers gleaming like miniature suns above roadside thickets and fence rows in the East, we may safely infer the spot was once an aboriginal or colonial farm. White men planted it extensively for its edible tubers. . . . As early as 1617 the artichoke was introduced into Europe, and only twelve years later Parkinson records that the roots had become very plentiful and cheap in London. The Italians also cultivated it under the name *Girasole Articocco* (sunflower artichoke), but it did not take long for the *girasole* to become corrupted into Jerusalem, hence the name Jerusalem artichoke common to this day. When the greater value of the potato came to be generally recognized, the use of artichoke roots gradually diminished."

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less. Putting their corn and other grains into large grass sacks, they throw them into these trenches, and cover them with sand, three or foure feet above the surface of the earth, taking it out as their needs require. In this way it is preserved as well as it would be possible to do in our granaries (p. 95).

The description "Of their Planted fruits in Virginia, and how they use them," which Captain John Smith wrote in 1606, is applicable also to this region.

The greatest labour they take, is in planting their corne, for the Country naturally is overgrowne with wood. To prepare the ground they bruise the barke of the trees neare the root, then doe they scortch the roots with fire that they grow no more. The next yeare with a crooked peece of wood they beat up the weeds by the rootes, and in that mould they plant their Corne. Their manner is this. They make a hole in the earth with a sticke, and into it they put foure graines of wheate and two of beanes. These holes they make four foote one from another;[1] Their women and children do continually keepe it with weeding, and when it is growne middle high, they hill it about like a hop-yard.

In Aprill they begin to plant, but their chiefe plantation is in May, and so they continue till the midst of June. What they plant in Aprill they reape in August, for May in September, for June in October; Every stalk of their corne commonly beareth two eares, some three, seldom any foure, many but one and some none. Every eare ordinarily hath betwixt 200 and 500 graines. The stalke being greene hath a sweet juice in it, somewhat like a sugar Cane, which is the cause that when they gather their corne greene, they sucke the stalkes: for as we gather greene pease, so doe they their corne being greene, which excelleth their old. They plant also pease they call Assentamens, which are the same they call in Italy, Fagioli. Their Beanes are the same the Turkes call Garnanses, but these they much esteeme for dainties.[2]

The season for planting, according to Belknap[3], was "when the leaves of the white oak are as big as the ear of a mouse." William Bradford, in his History of Plymouth,[4] says that Squanto "tould them excepte they gott fish and set with it (in these old grounds) it would come to nothing." *Mourt's Relation* (by Bradford and Winslow)[5] reports the same fact: "According to the manner of the

[1] "Four or five feet apart" is the spacing mentioned by W. Strachey, in *The Historie of Travaile into Virginia*, p. 116.

[2] *The Historie of Virginia, University of Glasgow Edition*, 1907, pp. 58–59.

[3] *History of New Hampshire*, vol. III, p. 93.

[4] *Massachusetts Historical Society Edition*, 1912, vol. I, p. 215.—A note by the editors on page 220 gives evidence for the fact that the Narragansetts "have good corne without fish." Possibly this may be the reason why we have never heard of the survival of the old corn-hills in Rhode Island.

[5] Ed. Dexter, 1865, p. 132.

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Indians, we manured our ground with Herings or rather Shadds, which we haue in great abundance, and take with great ease at our doores." Edward Winslow[1] says:

The seed-time beginneth in midst of Aprill, and continueth good till the midst of May. . . . Mays, which our Indians call *Ewachim*, . . . will not be procured without good labour and diligence, especially at seed-time, when it must also be watched by night to keepe the Wolues from the fish, till it be rotten, which will be in foureteene dayes; yet men agreeing together, and taking their turnes it is not much.

Concerning the inexhaustible supply of fish for their purposes, George E. Ellis[2] remarks:

The alewives were abundant, not only in the Taunton river, but probably in all the rivers along the coast. The early settlers speak of the great quantities of them in the two rivers at Plymouth; and a brook running into the Mystic, near Harvard College, is still called Alewife brook."

Roger Williams[3] gives still further details of interest concerning the Indians and their corn fields.

From thick warme vallies, where they winter, they remove a little neerer to their Summer fields; when 'tis warme Spring, then they remove to their fields where they plant Corne. In middle of Summer, because of the abundance of Fleas, which the dust of the house breeds, they will flie and remove on a sudden from one part of their field to a fresh place: And sometimes having fields a mile or two, or many miles asunder, when the worke of one field is over, they remove house to the other. [Crows were a nuisance, then as now.] Against the Birds the *Indians* are very carefull, both to set their corne deep enough that it may have a strong root, not so apt to be pluckt up, (yet not too deep, lest they bury it, and it never come up:) as also they put up little watch-houses in the middle of their fields, in which they, or their biggest children lodge, and earely in the morning prevent the Birds. . . . The Women set or plant, weede, and hill, and gather and barne all the corne, and Fruites of the field. . . . When a field is to be broken up, they have a very loving sociable speedy way to dispatch it: All the neighbours men and Women, forty, fifty, a hundred &c, joyne, and come in to help freely. With friendly joyning they break up their fields. . . . The *Indian* Women to this day (notwithstanding our Howes, doe use their naturall H- ves of shells and Wood."

Most of the descriptions thus far given speak of the hills either as being "scattered over the surface with the greatest irregularity,"

[1] *Good Newes from New England*, 1624, p. 62.

[2] *The Red Man*, 1882, p. 175.

[3] *A Key into the Language of America*, 1643, pp. 37, 46, 89, 100, 101.

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or more frequently as being placed four or five feet apart, implying regularly spaced parallel rows or ridges. In some parts of the country, however, the hills seem to have been placed much nearer together. Cyrus Thomas[1] quotes Sagard as describing the agriculture of the Hurons in 1623–26, saying that they dug a round place at every two feet or less, "every year in the same places and spots."

Summarizing, then, from these varied accounts from men who have seen the actual cultivation of Indian corn in New England by the aborigines, before they were touched by white influence, we have a definite picture of isolated hills, each built by itself, and cultivated in the same place, year after year. There was no suggestion of either plowing or breaking up an entire area in which they would plant as we do, but the fabrication of these separate hills, some two to four feet apart, usually in rows running one way.

There is some suggestiveness even in the method employed by the Indians in first clearing a piece of forest land, as described by Champlain, where they cut down the trees three feet from the ground, and then burned the branches upon the stumps, for such a method would leave available places for the separate hills only between the tree trunks on an average of some four or five feet apart. Such a first establishment of a new field, with the position of the hills conditioned by the stumps, would account for fields with the hills irregularly placed but about so far apart, and the fields with the hills in rows would be found either in naturally clear meadow land where there were never trees, or in a field long cleared, from which the stumps had been long rotted out. From the first coming of the whites, however, the Indians were very anxious to make use of the superior methods of the new comers, and in frequent deeds one can find mention of plowing up of so many acres of land as part compensation for a land purchase. Sometimes this plowing was offered by the whites as part of the inducement to get the Indians out of the vicinity, as at Northampton, to be explained later in connection with the removal of corn lands.

Concerning the agricultural implements employed, the shells

[1] "Handbook of American Indians," *Bulletin 30, Bureau of American Ethnology*, vol. I, p. 25.

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of clams and the shells and bones of other creatures with convenient hard parts are specifically mentioned; such as deer scapulae, the carapace of the Turtle, or that of the horse-shoe crab, this latter naturally only close to the coast, where Champlain observed it. Hoes and spades of stone were also used.[1] William Wood, in New England's Prospect, 1634, gives preference to cultivation by clam shells over that by European tools, in spite of the fact that the plough will "teare up more ground in a day, than their Clamme shels could scrape up in a month."[2] But here again the superior tools of the whites appear often as compensation in land purchases, especially "howes." It is also very probable that in the majority of cases the original tools were mainly of wood, such as we now find among the more primitive aborigines still extant.[3]

The pictures drawn from the earlier observers concerning the guarding against the two chief annoyances, crows and wolves, are very human. Is it possible that the "great many little houses scattered over the field" which Champlain saw in Boston were connected with scaring of the crows, and used perhaps as watch houses for the "biggest children" to lodge in during the critical time for this purpose, and also possibly to "keep the Wolues from the fish till it be rotten"?

2. Aboriginal Corn-fields on Assonet Neck, near Taunton

There are no less than four different localities upon Assonet neck in the town of Berkley, where the ground is covered with rows of earth mounds, which traditionally are known locally as Indian corn-fields, and the appearance of which corresponds closely to all descriptions of such grounds.

These are (1) in woods, on the Phillips farm;
(2) in woods, on the Delabarre farm;
(3) in the Bennett pasture;
(4) in the Coombs pasture.

[1] Cyrus Thomas, *loc. cit.*

[2] Cited in a note by the editors of the above mentioned edition of Bradford, vol. I, p. 219.

[3] For this see the recent article by Hough, "Corn cultivation among modern Hopi," *Proceedings of the United States National Museum*, vol. 54, pp. 235–296, especially the figures of Hopi agricultural implements of wood on pl. 19.

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Although it strikes one at first, especially one who has had little or no experience with surface indications of ancient human activities, as unlikely that the traces of ancient tillage could persist for so many centuries, yet a study of the early accounts of the methods of cultivating Indian corn as practised by the aborigines renders quite possible the persistence of such culture.[1] What is significant in these accounts as foundation for a belief in the long continued preservation of the corn-hills, even down to the present time, is the following group of facts: the same hills were used year after year; they were highly fertilized; there were annual additions to the hills, making them of considerable size. After the abandonment of cultivation, the fertility and previous culture of the hills would for a long time support abundant vegetation, whose decay would contribute to their size, and whose roots and leaves would bind together the soil and preserve it from erosion. The spaces between the hills, hard trodden by the laborers and never worked or fertilized, would support only the more meagre vegetation of our New England wild pastures; would furnish the channels through which water would drain; and would form natural pathways which men and cattle passing through the fields would follow for the most part, thus treading them down, in preference to walking up and down over the hills. Under these conditions, it is not surprising that in many places the original hills should never yet have disappeared. This antecedent probability is confirmed by our observations. The photographs that we present (figure 11) give conclusive evidence that we have to do with genuine remains of Indian cultivation. They are easily distinguished from any natural formations, such as merely "hummocky ground," or hillocks of bunch-grass and similar growths. Moreover, they present a wholly different appearance from that of abandoned corn fields of the whites. If white men ever used them, it could have been only for a brief period after they first began cultivation in old Indian grounds, and before they made use of the plough.

[1] For clearly discernible remains of cattle paths and a certain type of artificial pond vastly more ancient than these American Indian records, see A. J. and G. Hubbard, *Neolithic Dew Ponds and Cattle-Ways*, Longmans, Green and Co., 3d edition, 1916.

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These ancient corn-fields on Assonet neck are much more extensive than those of Northampton, where we have also observed them. This neck comprises about twelve hundred acres, lying in an angle at the confluence of the Taunton and Assonet rivers. It was one of the few places near Narragansett bay which remained

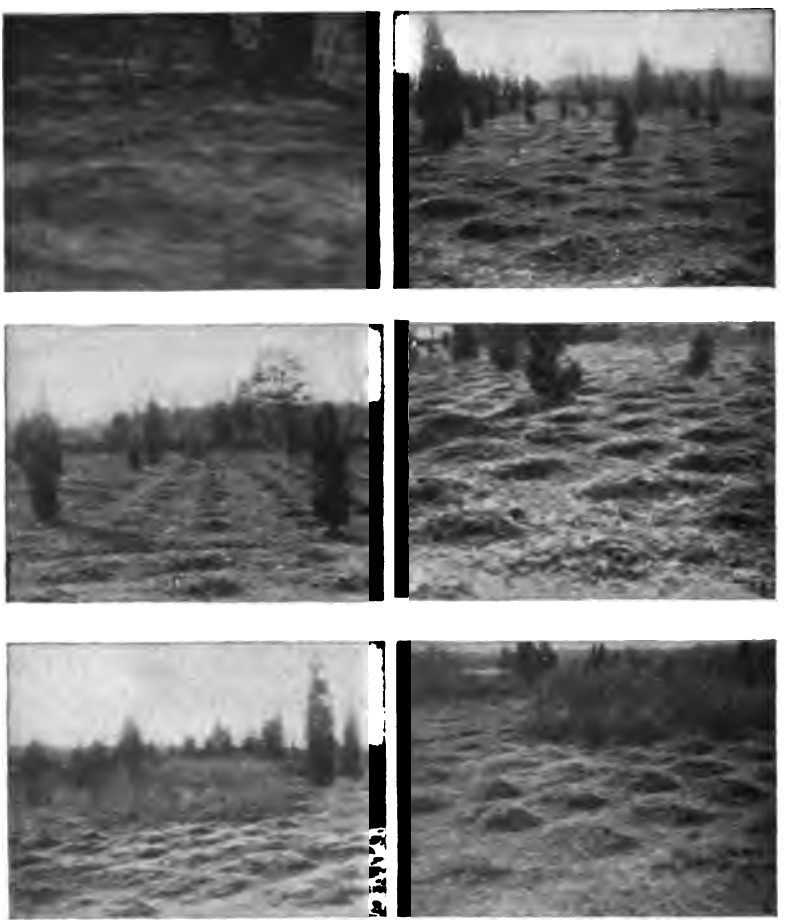


FIG. 11.—Indian corn-hills at Assonet neck, Mass. (1) In woods on Delabarre farm; (2) Coombes Pasture, looking south along rows; (3) Coombes Pasture, looking southwest; (4) Coombes Pasture, looking southwest along diagonals; (5) Coombes Pasture, looking southwest along diagonals: (6) Bennett Pasture, looking southwest along diagonals.

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in possession of the Indians until the close of King Philip's war. Several former sites of Indian villages have been discovered on it, and it was undoubtedly once extensively cultivated. There seems to be some evidence that it was little inhabited after the great plague of 1616. The passage of three hundred years, however, has left its ancient corn-fields still clearly marked in woods, pastures, and other places left untilled by the whites. Some families long resident on the neck still call them Indian corn-hills. Four separate localities, as enumerated above, are known to the writers, and there are very likely others which they have not yet explored. The total area known to be occupied thus is about thirty acres, which must include over eighty thousand hills.

The cultivation here, in every one of the four localities, was in the highest degree regular and orderly. The hills lie in almost perfectly regular parallel rows, nearly evenly spaced in the rows, so that the lines are straight not only in one direction, but also in the direction at right angles to that, and likewise in the two diagonals. Two of the localities are in woods. In such situations they are least clearly marked because the decay of leaves and fallen branches is slowly obliterating them by filling in the spaces between them. One of our photographs shows them, though not so clearly as they can be seen on the spot, underneath a white pine tree that is six feet in circumference at the base. The other two localities are in open pastures, offering better opportunities for study. In both of these places the direction of the rows, corrected for magnetic variation, is N. 20° E. and E. 20° S., uniform in the eight different measurements taken. The two localities are separated by about half a mile, one being on the Taunton river side of the neck, the other near Assonet bay. One field in the woods is on the top of a narrow ridge that runs about north and south in the middle of the neck, at a place where it is nearly level, fifty to sixty feet above sea-level. The other three are on ground that runs from this ridge down towards the water on the one side or the other in alternate slopes and levels, and for the most part on the level portions.

The spacing of the hills is not the two feet of the Hurons, but very nearly the same as that found by Lapham in Wisconsin,

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and by early observers in New England and Virginia. Seven lengths of about fifty feet each have been measured and the hills counted. In one of the two localities in pastures the hills were found to have an average distance apart of 3.75 feet, with a minimum of 3.25 and a maximum of 4.25; in the other, an average of 4.35 feet, with minimum of 4 and maximum of 5. The general average, then, is slightly over four feet, which is the distance of a long stride.

About a hundred of the hills, with the spaces between them, have been thoroughly dug over, in order to observe their structure and to see if Indian implements might be discovered. Many stones of all sizes were found within the hills. Evidently the Indians did not habitually throw these out, as one might naturally expect that they would have done. Very likely a moderate number of them was regarded as desirable, perhaps to keep the soil looser, perhaps even to facilitate digging. Many thin stones were found of such shape that they might well have been employed as hoes, but it was impossible to be sure whether they had actually served as such. Two well shaped oval hoes or spades of slate or similar material, one of them with notches at the sides, two broken arrowheads, and a small mortar such as might have been used for grinding mineral paint, were the only indubitable Indian artifacts discovered.

### 3. An Aboriginal Corn-field in Northampton

Attention has recently been directed to a rough field or pasture in Northampton, Mass., the surface of which is covered by small mounds set in definite rows, and evidently, both by the position of the field relative to traditional Indian sites, as well as by the intrinsic evidence of the field itself, a part of aboriginal corn-planting ground. The accompanying map, a part of one published in Trumbull's History of Northampton,[1] and reproduced from one issued by the Town in January, 1831, serves to show the main centers of activity, both of the Indians and of the English settlers (fig. 12).

Naturally the underlying features which have conditioned all settlement activities of both races are the courses of the two rivers

[1] Gazette Printing Co., Northampton, 1898, vol. 1, folder between pp. 16–17.

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FIG. 12.—Map of Northampton, Mass. Drawn in 1831.

of the vicinity, the Connecticut, which in early times was characterized by a great bend, or "ox-bow," incorporated in the main stream, and the smaller and still more sinuous stream, the Mill river. Throughout the area of this map, and further, the Connecticut is a

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very tortuous stream, abounding in extensive bends and loops. Of those shown here the most northern, extended towards the west and forming a long and narrow loop, is occupied by the settlement of Hadley, founded in 1659, which consists essentially of two streets, running north and south, connecting the two ends of the loop. Further down, near the southern limit of the map, as given here, is a second large loop or "ox-bow", more typical in its shape than the one at Hadley, and still at this time forming an integral part of the Connecticut river, its enclosed meadow, "Hockanum," being directly connected with the land upon the east side. Ten years after the drawing of this map, in the early spring of 1840, this ox-bow was cut off by the familiar action of rivers in such cases, and the new course connected the two banks at the mouth of the loop, leaving the ox-bow as a quiet lagoon, known locally as the "old-bed," and a famous fishing ground. A more careful scrutiny of the map at this point will show that in prehistoric times a similar ox-bow was cut off, situated farther to the west, and that its remnant, also in the form of a still more shrunken lagoon, cut into two pieces, was plainly seen in 1831.

If attention is now directed to the smaller stream, "Mill river," its earlier course, and the one used at the coming of the whites, as indicated here by the double dotted lines, caused it to form an extensive loop, almost surrounding "Fort Hill," and finally emptying into the prehistoric ox-bow. This stream, more than the larger Connecticut, caused the early English settlers much annoyance by frequently flooding its banks, so that, not only in the spring, but almost continually in some seasons, the meadows between this loop and the Connecticut were under water. This soon caused the settlers to dig a new course directly to the Connecticut (1720), cutting off one whole side of the loop, and effecting a juncture directly to the side of the ox-bow as in this map. Since the severance of the ox-bow, ten years later, the lower course of Mill river was again changed, and it now empties into the main Connecticut.

The traditional site of the Indian town, where the purchase was probably made for the present town of Northampton by John Pynchon, was the sightly piece of raised ground lying in the

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little loop of Mill river, and it was evident from the outset that so close a proximity to the place where the English designed to build would be in many ways unpleasant. It was therefore definitely stipulated, and so stated in the original deed of purchase, that the Indians were to remove their settlement here, in the loop of Mill river, and locate upon the east side of the Connecticut the following year (1654), the English to plow up for them sixteen acres of meadow land there for the planting of their corn. The Indians were allowed to keep their present corn-fields for the year following, 1654, but after that time they were to remove wholly to the east of the Connecticut, and leave the English finally free from them. The original text, which seems to have been rather unusual in its definiteness, runs in part as follows:

. . . The Aforesaid Indians and in pticular Wawhollowa, Nenessahalant, and Nassachohe beeing the Sachems of Nanotuck doe for themselues and with the Consent of the other Indians and owners of the sd Groundes, sell, giue, and Grant vnto John Pynchon of Springfeild and to his Assignes for and in the Consideràtion of one hundred fathum of Wampam by Tale and for Tenn Coates (besides some small gifts) in hand to the said Sachems and owners, All the land Aforesaid as [by] these presents haue bargained, granted and sould to the [said] Pynchon all and singuler the said landes free from all Cumbrances of Indians provided the said Pynchon shall plow vpp or cawse to bee plowed vpp for the said Indians Sixteene Acres of land on the Easterly side of Quoneticutt River which is to bee donn sometime next summer 1654 And in the meane time viz the next spring 1654, the Indians haue liberty to plant ther present Corne feildes, but after that time they are wholly to leaue that West side of the river. And not to plant or molest y^e^ English ther.

All the said Premises the said Pynchon and his assignes shall haue and enjoy Absolutely and Clearly forever [from] all in Cumbrances from any Indians or their corne feilds.

Thus it is clear that the Indian corn-fields included within this loop of the smaller river were harvested by them for the last time in the fall of 1654, while during the next few years the Indian people themselves were much in evidence, living for the most part in friendly relations with the whites, paying an occasional fine for drunkenness, or breaking into a mill and stealing "divers tool and meal." In ten years, however, probably fearing the Mohawks, who had then become occasional visitors to this valley, the local

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Indians applied to the English town for "a gift of land on which to build a fort" [Trumbull, *History of Northampton*, vol. I, p. 176.] This was granted under certain rather stringent conditions, but there is no definite record as to the location of this later fort. Although some of the local historians seem to mix this up with the fort in the loop of the river of previous occupancy, there is good evidence that such was not the case, as this latter territory had already been apportioned to several English settlers. The most likely site for this later settlement was at the bend of the river west of Hadley, on the way to Hatfield, marked on the extreme north of the accompanying map as "Fort Plain." In any case this interval of Indian occupancy was of short duration, for at the outbreak of Philip's war in 1675 the Indians withdrew from the entire region, never to return.

This, then, is the history of a definite Indian village, the one almost coincident with the first center of Northampton. Traditionally it consisted of a "fort" or palisade, which is said to have been on the bluff at the north-east end of the elevation, on grounds now owned by Mr. Frank Lyman. This location is a very sightly place, looking off towards the east with a long stretch of meadows, and Mts. Holyoke and Tom in full view. Almost at one's foot the original channel of the Mill river is distinctly visible, cutting off the village site from the plains below, and emptying finally into the side of the oldest ox-bow. Since, in this region at least, there are always to be expected in each Indian settlement three associated sites, the village, the corn planting grounds, and the burying ground, one is tempted *a priori* to make a guess as to the most probable location for the two last. Burying grounds hereabouts seem to show no fixed relationship to the village site, and their discovery is a matter of chance, bones being found by plowing or other excavation; but if any bones have ever been unearthed here their tradition has long been lost. That such has been the case is very probable, as South St., one of the most populous residence streets, runs lengthwise through the entire elevation, and, as this is also one of the oldest streets, any such bones would have been found early, when Indian bones were sufficiently frequent to have caused no special mention.

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As for the corn-fields, however, their location could have been only in one general region, along the north and west side of the elevation. The land drops precipitously almost alongside the street, and the lower level extends a long distance as a rather narrow meadow, enclosed within the loop, and of very easy access to the high land. On the east and south of the high land, on the other hand, the meadows were cut off from the settlement by the loop of the river, and in addition to this the meadows were frequently flooded, previous to 1720 when the river was put into a new channel.

It is precisely upon this northern and western meadow, practically within this river loop, that there are situated the remains of the Indian corn-field which is the subject of this portion of the paper. It lies in a somewhat sunken meadow, shut off on one side by a railroad embankment and on the other by the bluff leading up to the high land of South street. About a hundred years ago a canal was constructed between Northampton and New Haven, which ran immediately at the foot of the bluff, as still shown on the map of 1831, and naturally this part of the field is free from all traces of cultivation. But between the old canal bed and the railroad embankment, which here runs parallel to it, extend some three thousand elevated mounds in nearly perfect rows, presenting the general appearance of the accompanying photographs.

One first gains access to this field (fig. 13, 1), which is low and sunken, by crossing over the railway, and coming down to it upon a long narrow mound, clearly visible in the foreground, and giving at first much the appearance of an intentionally constructed driveway. A moment's study of the land back of this, and upon the other side of the railway, shows it to be nothing but the end of a natural slope, the result of glacial action and due wholly to natural causes. When the observer has stepped wholly off this mound and stands upon the lower level (fig. 13, 2 and 3) he stands among the corn-hills and sees them in very good parallel rows, in a general NE.–SW. direction. The houses upon the bluff in front are placed along the road, South St., which traverses the bluff lengthwise, and are here situated where the bluff is very narrow, scarcely wider than the



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FIG. 13.—(1) Indian corn-hills in meadow between Earle and South Streets, Northampton, Mass. Approach from railroad embankment, looking southeast; (2) Indian corn-hills in Northampton. Observer standing just to the east of the large mound shown in (1); (3) Indian corn-hills in Northampton. Observer a few steps further east than in (2), in alignment with the rows of corn-hills shown in (3); (4) Hills in the middle of the field; in the immediate foreground some of the elongated mounds; (5) a detail of the Northampton corn-field showing growth of hardhack (Spirea); (6) A view of nearly leveled hills lying near the railroad embankment; the observer is looking northwest, just the opposite direction from that from which the five others were taken.

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street and the two rows of houses. The more expanded portion of the higher land is to the observer's left (east), and runs from here nearly half a mile before the end of the bluff is reached, and the presumable site of the palisade.

The arrangement of the separate hills does not quite correspond to the double alignment of the fields of Assonet, nor is at all like the perfectly irregular order described by Lapham, which may perhaps have for its cause the method of planting on partially cleared forest areas. It is an alignment in one direction only, fairly straight rows running from northwest to southeast, but with the distance between the individual hills of each row so irregular that for the most part the hills do not line up when viewed across the longitudinal rows, either at right angles or diagonally. In a few spots there is such a chance alignment, but it does not continue more than a few hills, and is plainly a chance arrangement.

The rows were evidently run by eye, without the use of a line, as they do not keep quite straight, but get to curving quite perceptibly, all in one direction, when looking down the entire field. The average distance from row to row is a little more than 3 feet; we had a meter stick to measure them with and the meter stick would about span them from the center of one hill to that of the nearest one in the adjacent row. The distance from one hill to the next in the same row was approximately the same, although the fact that definite cross-rows could not be perceived was a clear proof that there was no constancy in this direction. Down in the south corner of the field, where it was the most overgrown by the spirea bushes, the distances were rather greater than elsewhere, that between rows being fully four feet. In one or two places, where a curving of one or two rows had left a chance space between the rows, a single hill, or perhaps two hills, were squeezed in out of alignment, evidently with the utilitarian purpose only of getting in all the corn-hills possible within that area. At the present time the entire meadow is rather wet for successful corn planting, but the very next field to it on the east was planted to corn only last year, and the crop was apparently a good one. This field was lower, and has in the middle some standing water at present, but even

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here the spirea tops, sticking up through the water, are as definitely in rows as elsewhere. Furthermore, several changes within historic times must have effected some changes of level over the entire region, such as, for instance, the building of a dam in Mill river a little below this about 150 years ago, and probably also the formation of the solid railroad embankment in 1840. There is decided indication of spring inundations over that part where the corn-hills are not now very apparent, and we may readily assume that such profound surface changes as have taken place over this region, dams, embankments, and so on, have brought about changes in the conditions over the surface, and in the action of the water.

While in general the separate hills are fairly uniform in size and shape, conical mounds of perhaps two feet in diameter, and 8 to 12 inches in height, there are certain regions where the mounds are irregular in shape, and prolonged laterally, generally obliquely, to give the appearance of the conventional gravemound (fig. 13, 4). In one place, and involving some two rows, these oblong mounds come in succession and involve twenty to thirty hills in all. In one portion of the field the ground is covered with hardhack, *Spirea tomentosa*, which, as is its habit, picks out the elevations and leaves the lower levels bare (fig. 13, 5). Here, even in November, when the bushes are represented by bare and dry stalks the alignment is well seen by following the tips of the dry bushes, and thus where otherwise the area would be a hopeless tangle, the definite alignment of the corn-hills is as clearly marked as elsewhere.

In some places, whether the mounds are quite free from the spirea, which must have somewhat of a protective effect upon them, or from some other reason, such as inundations, the mounds are being gradually worn down, and in some directions or in some lightings are but dimly seen (fig. 13, 6). The particular ones shown in this figure lie near the railroad embankment and they may have suffered from some of the circumstances incident to this work. Finally, over the easternmost half of this field the corn-hills have all but disappeared, although there are sufficient traces here and there to assure one that this tract, as well as the part where the preservation is more perfect, was once included in the original corn-planting lands.

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If, now, by the help of this corn-field, and what there is left by tradition, and by our knowledge of former courses of the rivers and the water level in general in this region, we should attempt to reconstruct in our mind's eye the appearance of this little region just previous to the advent of Master John Pynchon, Real Estate Dealer from Springfield, we get a most pleasing picture of an ideal aboriginal settlement, exceptionally well protected by nature from hostile forces and furnished with just the advantages desired. A bluff, sufficiently high to overlook the surrounding country on all sides, tapers off behind into a narrow neck, easily guarded, and is enclosed by an almost complete loop of a fairly swift stream, large enough to furnish a decided barrier to a band of hostile men. Aside from the highland the loop also included an ample stretch of good corn land on a clear meadow, protected by its position from the frequent floods from both rivers, which in front and on the lower (southern) side continually convert the meadows lying without the loop into an unbroken sheet of water. When one sees this palisaded village, standing up out of this vast expanse of flooded meadows, one thinks forcibly of Judd's derivation of the word Nonotuck (with its dialectic forms Norwattock, Nolwottogg, etc.), as "in the midst of the waters," from Natick *noeu* or *noau* (in the midst of), and *tuck*, a stream or river.[1] Unfortunately, however, as in so many other cases, a romantic significance given for an Indian word cannot stand the common-sense explanation of a man familiar with the spoken tongue of the Algonkians, in such nearby tribes as the Penobscot or Passamaquoddy; and in spite of the emotional appeal of this "Village-in-the-midst-of-the-river," candor compels us to state that our friend Dr. Frank G. Speck, who is not only a philogogist of much note, but who also lives on the best terms with the tribes just mentioned, and speaks their language with perfect readiness, says that *nonotuck*, in all its different dialectic forms, is simply to this day the common term for "up-river," a term applied to any thing, island, tribe, or whatever it may be, in the direction

[1] Sylvester Judd, *History of Hadley*, p. 114. Trumbull's Natick Dictionary gives the word *noeu*, with the meaning of "in the middle, the midst," and Eliot uses it in this sense in his Bible, Joshua, chap. XIII, verses 9, 16.

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designated. This was hardly the name by which the people of this village designated themselves, but as Springfield, down-river from Nonotuck, was the first locality about here known to the whites, who opened a trading-store in 1636, having reached the Connecticut at this place across country, through Quinsigamond (Worcester) and Quabaog (Brookfield), they would first hear of these up-river tribes from the local Indians that came about Master William Pynchon's store, who would naturally speak of them as *nonotuck*, the people up-river. When, twenty years afterwards, John Pynchon, son of William, bought the land of these up-river Indians, a part of his stipulation included the promise on the part of these same Nonotucks to remove across the big river, and leave the land in entire possession of the English.

### 4. Conclusion

There can be no doubt, then, that these "Indian corn-hills," still extant and plainly visible not only in these places that we have described but probably also in many others, are exactly what local tradition in some cases holds them to be—genuine relics of Indian cultivation of the land. Their general appearance proves it, conformable as it is to that which the hills in actual cultivation by the Indians must have presented, and differing wholly from that of fields in which any kind of white men's crops have been grown. The known identity in locality of the present remains in Northampton with the site of the ancient Indian corn lands is another convincing proof. It is easily understood how conditions have insured their survival, in many cases with probably little change in appearance from that which they presented when they were first abandoned, two and a half to three centuries ago.

It is likely that, if the fields in which they lie continue as waste land or used for no other than pasturage purposes, they will still be easily observable for at least as many centuries to come. We have found them varying considerably in size and shape and distance apart; and the authorities whom we have quoted describe still further variations. In size and shape, they are usually low roundish individual mounds, less than two feet in diameter, rising above the

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intervals between them from eight to twelve inches in the best preserved examples, and thence down now to a mere trace in cases where destructive influences have affected them. Sometimes, however, a number of them are joined together into a continuous mound; and apparently Lapham observed a type where entire rows formed continuous ridges without individual hills, though his description is not definite enough to make this sure.

As to their distance apart, there was evidently much difference in practice among Indians of different localities, though perhaps a considerable uniformity in any one locality, unless conditions varied, as for instance in the use of land newly cleared of trees as against that which had always or long been clear. At one extreme are the hills that Lapham describes as scattered over the surface with the greatest irregularity. The closest regular spacing seems to have been that of the Hurons, two feet or less. A third method of spacing was that of Lapham's parallel ridges about four feet apart, apparently without individual hills. Most of the other quoted authorities give the interspaces as three to five feet, without mention of whether they were irregularly placed or formed definite rows, and in the rows continuous ridges or separated hills, yet almost certainly the latter except in newly cleared land. Where definite rows of individual hills were formed, they were sometimes aligned in one direction only and no attention was paid as to whether or not they formed straight lines with one another in the direction at right angles to that; but sometimes great care was used to arrange them straight and true in both main directions and thus also in both diagonals. At Northampton there are regularly parallel, though not necessarily entirely straight rows a little over three feet apart, singly aligned because within the rows the spacing of the hills was irregular. Finally, most definite and orderly of all, on Assonet neck the rows run everywhere perfectly straight (unless rocks or other obstacles interfere), doubly aligned, with an almost constant distance between hills of a little more than four feet in both directions.

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# SOME CONCLUSIONS AND SUGGESTIONS REGARDING THE POLYNESIAN PROBLEM

By EDWARD S. HANDY

IN view of the organized attack on Polynesian problems, anthropological and otherwise, which is on the eve of being launched at this time, it is perhaps the duty of those who feel that they have conclusions and theories which may possibly be useful or stimulating to others who are working or thinking in the same field, to put before their co-workers these suggestions, even though the conclusions are necessarily of a tentative nature. The conclusions stated below are based on literary research into certain phases of Polynesian culture in which the writer has been engaged for several years. While it is felt that the information derived from these sources is sufficient to warrant the drawing of such conclusions, it is hoped that these will be clearly understood to be tentative suggestions based on the limited data now available. The information now at hand will probably dwindle into insignificance before the more plentiful and accurate data which it is hoped that the next few years' work in the area will place before the scientific world.

Unfortunately lack of time and space makes impossible the presentation of the evidence which it is believed supports the conclusions which follow. These conclusions are the outgrowth of a somewhat exhaustive study of the literary sources of information with regard to the area, in the course of which was accumulated a considerable amount of material which cannot even be referred to in an article of this kind. In most cases also it has been impossible to go into explanations of the lines of reasoning which have led to the conclusions. A few references chosen from a considerable number may aid those who are interested in judging for themselves as to whether the conclusions are justified.



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I

The places of burial of sacred chiefs were places of public worship in Hawaii, the Society islands, the Marquesas, Tonga, and New Zealand. Information regarding this is lacking in the Cook group. In Easter island worship seems to have been conducted before the great image platforms which were used for burial. From Samoa evidence which would indicate that chiefs' tombs were places of public worship is lacking.[1]

In historic times it appears that places of public worship, or temples, were frequently, though not always, used for burial purposes.

It is believed that the prototype of the stone tomb and temple forms of Hawaii, the Society group, the Marquesas, and Easter island was a tomb form. The rudimentary type of this tomb-temple is probably to be found in the tombs of the kings in Tonga, consisting of superimposed earth platforms faced with stone blocks.[2] These platforms may have originated in the simple earth mound used here for burial in historic times, or this earth mound may have represented a degeneration from a stone tomb.

The following temple and tomb forms, derived from this Tongan prototype, were found in those island groups which utilized stone construction, and concerning which we have adequate information.

[1] See for Hawaii: W. Ellis, *Polynesian Researches*, vol. IV, pp. 164–6, London, 1853. James Cook, *The Three Voyages of Captain James Cook Round the World*, pp. 882–3. London, 1842.

Society Group: Cook, *op. cit.*, p. 771.

Sir Joseph Banks, *Journal of the Right Honorable*, etc., p. 175. London, 1896.

J. A. Moorenhout, *Voyages aux Iles du Grand Ocean*, vol. I, p. 470. Paris, 1837.

Marquesas: A. Baessler, "Reise in östlichen Polynesien," *Verhandlungen der Berliner Gesellschaft für Anthropologie, Ethnologie und Urgeschichte*, 1896, p. 464.

Tonga: W. Mariner, *An Account of the Natives of The Tonga Islands in the Pacific Ocean*, compiled by John Martin, pp. 385–387.

New Zealand: R. Taylor, *Te Ika a Maui; or, New Zealand and its Inhabitants*, pp. 98–9, 174, 183, 208. London and New Zealand, 1870.

Easter Island: Paymaster W. T. Thompson, "Te Pito Henua, or Easter Island," in *Report of the U. S. National Museum for the year ending June 30, 1889* (published 1891), pp. 470–1, 499.

[2] *A Missionary Voyage to the South Pacific Ocean . . . in the Ship Duff*, etc., pp. 278–9. London, 1799.

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In the Society group there were early platform, and later pyramidal types of maraes; the pyramid growing out of the superposition of a number of platforms. In Hawaii were found early platform and pyramidal types, and later walled heiaus with inner compartments. In the Marquesas there developed the platform ma'ae, sometimes consisting of several terraces running up a hillside. And in Easter island there were the stone platforms on which the great images stood, the platforms being stepped on the landward side.[1]

The variation in form of the tomb-temple in the several groups may be explained for the most part by local environment and political development. Thus, the influence of environment is to be seen best in the Marquesas where the necessity of accomodating the temples to the abrupt slopes of the valleys produced the terrace forms. The effect of political development may be seen in Hawaii, where the organization of state and cult had attained its greatest development. This led to the exclusion of commoners from temple ceremonial and to the development of the great walled heiaus.

The use of large stone construction in tombs and temples seems scarcely to have touched the Cook group, and not to have influenced New Zealand at all. Thus, large stone construction was found to have been confined to the northern and central part of the area.

Certain important features connected with tomb-temples occurred pretty generally over the whole area, including New Zealand. The first of these was the association of the places of worship and places of burial which was discussed above.[2] Other features of importance were the following.

[1] Society Group: Paul Huguenin, "Raiatea la Sacrée," *Bulletin de la Société Neuchateloise de Géographie*, Tome XIV, p. 164. Neuchatel, 1902. *Duff's Voyage*, p. 304.

D. Tyerman, and G. Bennett, *Journal of the Voyages and Travels*, etc., compiled by James Montgomery, pp. 176, 194–5. Boston and New York, 1832.

Hawaii: Cook, *op. cit.*, p. 968.

A. Fornander, *An Account of the Polynesian Race*, vol. II, p. 6. London, 1878–1885.

Ellis, *op. cit.*, vol. IV, pp. 97–8, 116.

Marquesas: Dr. Tautain, "Notes sur les Constructions et les Monuments des Marquises," *l'Anthropologie*, VIII, pp. 667–71.

Easter Island: Thompson, *op. cit.*, pp. 499, 502.

[2] This was, of course a natural concomitant of the ancestral cult which constituted a fundamental element in the worship everywhere.

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In all the groups there was found to be a sacred area, which was in front of, or around, the sacred place. The sacred place usually consisted of a mound, platform, or pyramid. A sacred enclosure was formed by surrounding this area with a fence or stone wall in all the groups concerning which we have information, *viz.*, Hawaii, the Society group, the Marquesas (apparently only sometimes here), Easter island, Tonga, and New Zealand.[1]

Within and without the sacred enclosure were sometimes one, sometimes a number of sacred houses used for different purposes: protecting the tomb, sacred relics, images or other representations of deity, paraphernalia; for housing priests; and so on.

Sacred groves were associated with places of burial and worship in Tonga, Samoa, the Society and Cook groups, the Marquesas, and New Zealand.[2] The fact that these sacred groves were not found in Hawaii may be due to environment. This would certainly be capable of explaining the lack of them in Easter island.

At the back of the sacred place in Hawaii, the Society group, the Marquesas, and the Cook group was a sacrifice pit into which remains of offerings were thrown.[3] A ditch at the back of the

[1] Hawaii: D. Malo, *Hawaiian Antiquities*, pp. 211–14. Honolulu, 1903.
Society Group: *Duff's Voyage*, p. 304.
Marquesas: Pedro Fernandez de Quiros, "The Voyages of . . . 1595 to 1606," *The Hakluyt Society*, Series II, vol. XIV, p. 60. London, 1804.
H. Melville, *Typee*, p. 59. London, 1904.
Easter Island: Don Felipe Gonzalez, "The Cruise of . . . to Easter Island," 1770–1771. *The Hakluyt Society*, Series II, No. XIII, p. 136. Cambridge, England, 1808.
Tonga: *Duff's Voyage*, pp. 278–9.
New Zealand: S. Percy Smith, "The Lore of the Whare-wananga," etc., *Memoirs of the Polynesian Society*, vol. III, p. 89. New Plymouth, N. Z., 1913.
See also Taylor, *loc. cit.*

[2] Tonga: Cook, *op. cit.*, p. 419.
Samoa: G. Turner, *Nineteen Years in Polynesia*, p. 240. London, 1861.
Society Group: Ellis, *op. cit.*, vol. I, pp. 341–2.
Cook Group: W. W. Gill, *Historical Sketches of Savage Life in Polynesia*, p. 195. Wellington, 1880.
Marquesas: Melville, *op. cit.*, p. 147.
New Zealand: Taylor, *op. cit.*, pp. 98–9.

[3] Hawaii: Malo, *op. cit.*, pp. 211–14.
Society Group: M. de Bovis, "État de la Société Tahitienne a l'Arrivée des Europeans," *Revue Coloniale*, 1855, pp. 44–7.

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tomb of a Tongan chief, which is described by Cook, probably corresponds to this sacrifice pit elsewhere.[1] There is interesting indirect evidence which suggests that the sacred latrine in New Zealand[2] may also correspond to these pits.

There was definite orientation in the Cook group[3] and New Zealand,[4] temples or sacred buildings facing the east. In Hawaii temple enclosures seem to have been orientated to different cardinal points in those instances in which we have information regarding this.[5]

There was too much variation with regard to houses, altars, images, drums, ovens, certain boards erected in memory of chiefs, and some other features associated with places of worship, to allow of a discussion of these here. The oracle tower in Hawaii appears to have had no correspondence elsewhere in the area.[6] The mere mention of these as features which were associated with places of worship in various parts of Polynesia may, however, be suggestive.

## II

Stone slab seats associated with sacred places, sacred chiefhood, and the ancestral cult,[7] were found in New Zealand, on Rarotonga

Marquesas: Tautain, *op. cit.*, p. 688.

Cook Group: W. W. Gill, *Myths and Songs, from the South Seas*, p. 295. London, 1876.

[1] Cook, *op. cit.*, p. 716.

[2] Smith, *op. cit.*, p. 88, note.

[3] Gill, *Historical Sketches*, etc., p. 32.

[4] White, John, *The Ancient History of the Maori*, vol. I, p. 5. Wellington, 18— to 1890.

[5] Malo, *op. cit.*, p. 214.

A. Kraemer, *Hawaii, Ostmikronesien, und Samoa*, p. 106. Stuttgart, 1906.

C. Wilkes, *Narrative of the U. S. Exploring Expedition*, vol. IV, p. 100. Philadelphia, 1485.

Ellis, *op. cit.*, vol. IV, pp. 97, 116.

[6] Malo, *op. cit.*, pp. 211, 222.

[7] It will be of importance to determine whether these stone slabs used as seats were identified with the slabs which lined the graves of chiefs in Tonga and Samoa. Dr. Tozzer has made the interesting suggestion that the platform which it has been supposed was the prototype of the temple forms may itself have been in origin an elaborated seat of sacred chiefs. There is evidence to support this suggestion. This is a very important point: the proof of the identity of origin of the slab seats and the platform would, as is easily to be seen, necessitate a total abandonment of most of the conclusions stated in the second part of this paper.

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(Cook group), Niue, Samoa, the Society group, and the Marquesas.[1] We know of no evidence of the use of such seats in Tonga or Hawaii. This usage was, therefore, of importance in the southern groups where stone construction was not found, *viz.*, New Zealand and the Cook group. Furthermore, stone seats were not found in association with chiefs or sacred places in Tonga or Hawaii where large stone construction was of great importance. Hence it was concluded that this use of stone slab seats belonged to one cultural stratum, while the utilization of large stone in temple construction belonged to another.

In connection with this there is some interesting and very suggestive evidence with regard to Hawaii. The Pohaku o Kane, or stones of Kane, were here upright stones of varying sizes which were venerated by the lower classes.[2] It seems possible that the Pohaku o Kane originally corresponded to the stone seats under discussion. If this proves to be so, will it not indicate the submergence in Hawaii of that cultural stratum of which the veneration of such slabs as seats of sacred chiefs was typical? A number of other bits of evidence lend support to this theory. It is impossible, however, to enter into a discussion of these here, because they have grown out of the study of certain phases of the religion of Polynesia as a whole, which would have to be described with more thoroughness than space allows at this time. But it may be said in passing that careful study of certain matters in Hawaii would be expected to throw much light on this question. Some of these features in Hawaiian culture are the use of face tattooing by the Kauwa, or lowest class, exclusively; the fact that these Kauwa were also called

[1] New Zealand: S. Percy Smith, *Memoirs of the Polynesian Society*, vol. III, pp. 88–9.
Cook Group: S. Percy Smith, "Arai-te-Tonga, the ancient Marae of Rarotonga," *Journal of the Polynesian Society*, vol. XI, p. 174; vol. XII, pp. 218–20.
Niue: S. Percy Smith, "Niue Island and its People," Part II, *Journal of the Polynesian Society*, vol. XI, p. 174.
Samoa: G. Turner, *Somoa, a Hundred Years Ago and Long Before*, p. 23. London, 1884.
Society Group: A. Baessler, *Neue Südsee Bilder*, pp. 119–20. Berlin, 1900.
Marquesas: LeP. Mathias Garcia, *Letters sur les Iles Marquises*, p. 72. Paris, 1843.

[2] W. D. Alexander, *A Brief History of the Hawaiian People*, pp. 37, 44. New York, 1892.

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Aumakua, the term used for ancestral deities of the private cult; the apparent use of flexed burial by the lower classes only; the use of the kuahu shrine and employment of shamanistic workers exclusively, and the use of the oven largely, in private ceremonial; evidences of former cannibalism; and so on.

To sum up, therefore, the hypothesis is presented that the cultural stratum, of which the use of stone slab seats was characteristic and which was represented by the chiefs in New Zealand and elsewhere in the southern and central part of the area, was submerged in Hawaii, being represented there by commoners; and that another cultural stratum, of which the use of large stone construction was characteristic, was spread over the central region and Hawaii but influenced the Cook group and New Zealand only to a very slight extent.

## III

An analysis of the elements constituting the religion of Polynesia and a study of these with regard to their distribution led to the following grouping of these in association with the use of stone slab seats and large stone construction. Unfortunately time and space do not allow me to give my reasons for this classification, to present my evidence, or even to give adequate references. How much of this classification will stand, how much of it will be found erroneous in the light of future information, is unknown. It is offered at this time, however, in the hope that it may be suggestive and perhaps stimulating to others interested in the Polynesian problem, and in the problems of the other related areas to the westward where lie the routes by which the Polynesians must have migrated.

Simply for the sake of having some designation for the peoples to whom belonged these several cultural strata, those who brought the use of stone slab seats have been called Slab Users, and those who utilized stone construction, the Stone Builders. The Slab User elements are to be found most clearly defined in New Zealand, while the Stone Builder elements are dominant in Hawaii. In the central region they are combined in various ways.

It may be pointed out that certain important elements stand

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out in very distinct contrast as characteristic of the religions of the northern and southern extremes of the Polynesian area. Around these as nuclei were grouped other elements which seemed to be associated. Thus we find:

In Hawaii: stone construction, seasonal ceremonial in which a sacred king takes a priestly part, the ceremonial taboo, in general a thoroughly organized and ordered worship. These are totally lacking in New Zealand.

In New Zealand: stone slab seats, sacred groves, the veneration of skulls, shamans, the use of coercive spells in connection with public enterprise, planting and harvest a ritual performance, the Hawaiki belief. These are totally lacking or entirely secondary in Hawaii.

*Elements typical of the Slab Users are the following*:

(1) The veneration of slabs associated with ancestors and sacred chiefs, these slabs being generally used as seats by chiefs.
(2) Sacred groves.
(3) Sacred chiefs functioning in the public ancestral cult.
(4) Ancestral deities, both public and private. The veneration of skulls and other ancestral relics.
(5) Methods of disposal of the dead: exposure, flexed inhumation in a sitting posture, use of canoe coffins, secondary disposal of skeletal remains in caves. The placing of offerings of food and weapons with dead bodies.
(6) Funerary feasts.
(7) Survival of head hunting in the preservation of enemy skulls and heads.
(8) The belief in incarnation of ancestral spirits in animate and inanimate objects.
(9) Omens from animal movements.
(10) Divination by gazing into liquids, by possession, and in trance.
(11) Shamans: inspirational diviners, necromancers, magic workers. A great use of witchcraft in public and private application, employing coercive spells.
(12) The use of genealogies as religious formulae.
(13) The belief in the similarity of spirits of natural objects to man's spirit.

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(14) A more primitive form and use of the dance: war dances, paddle dances, spear dances, dances by widows of warriors.
(15) The work of planting and harvest a ritual performance.
(16) Those types of taboo which are particularly associated with the ancestral cult.
(17) Rahui, prohibition or restriction by means of badges or signs.
(18) The use of water in purification ceremonies.
(19) The use of the oven in public and private rites.
(20) The belief in Hawaiki, an origin-land to which the spirits of men returned.
(21) Stratified heavens of myth.
(22) Tattooing.
(23) Cannibalism.

*Elements typical of the Stone Builders*:

(*a*) The use of large stone in the construction of tombs and temples. (See No. 1 above.)
(*b*) Embalming.(?) The use of tombs. (See No. 5 above.)
(*c*) Violent mourning, dissipation after a sacred king's or chief's death, hired weepers, the singing of eulogies. (See No. 6 above.)
(*d*) Special rites for deifying great men.
(*e*) General or ceremonial taboo. (See No. 16 and No. 17 above.)
(*f*) The worship of the great gods of myth in the public cult. (See No. 4 above.)
(*g*) Divining by breaking objects and observing the scattering of fragments. (See No. 9 above.)
(*h*) Haruspication. (See No. 10 above.)
(*i*) An organized priesthood, the temple priests or directors of ceremonial being allied to the chiefs or kings. Inspirational diviners, necromancers, and magic workers relegated to a secondary position. (See No. 11 above.)
(*j*) Craftsmanship: the development of trades in the hands of master-craftsmen who were priests of the rituals of their trades.
(*k*) True prayers, supplications, associated with the offering of sacrifices. Human sacrifice. (See No. 11 and 12 above.)

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(*l*) The belief in man's possessing a soul peculiar to himself, and in nature's being animated by nature spirits differing from men's souls. (See No. 13 above.)

(*m*) A generation or fertilization cult expressed in seasonal ceremonial; dancing in which sexual abandon played a part; the functioning of sacred chiefs or kings in a priestly capacity in first fruits rites, and a belief in the intimate connection between the sacred chief or king and the growth of things and prosperity. (See No. 15 above.)

(*n*) Organized dancing and singing as part of public ceremonial. (See No. 14 above.)

(*o*) The belief in a lower hades for the unfortunate, and an upper paradise for the fortunate. (See No. 20 above.)

(*p*) In general this stratum was represented by a better organized and higher type of worship.

It may be remarked in connection with recent discussion of the occurrence of sun worship in Polynesia[1] that no evidence was found which would, in the opinion of the writer, warrant the assumption that a sun cult was ever a basic element in Polynesian worship.

We must leave untouched for the present the questions as to whether the Slab Users or Stone Builders were the first to colonize the area; whence they came, and when; and with which of the waves of colonization outlined by other students of the area they would probably be identified. It may be found as our store of accurate knowledge increases that many of the elements mentioned above are wrongly classified. But it is my belief that the work of the next few years will prove at least the general conclusions which underlie this tentative grouping of elements; that the greater part of the culture of Polynesia was made up of the combination of the elements of two great cultural infusions; that it will be possible to resolve the cultural complexes of the various island groups into constituent elements which will be found to have been originally characteristic of these two strata; and that these groups of elements will be capable of being traced back through the regions to the west-

[1] See W. H. R., Rivers "Sun Cult and Megaliths in Oceania," *American Anthropologist*, N. S., vol. XVII, pp. 431–445.



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ward to the cultural sources whence they were derived. Mention should be made in connection with this statement that there is evidence in Tonga and Samoa of the presence of a later infusion, and that there occur here and there in the area sporadic intrusive elements.

It is felt that all these questions must be left more or less in the balance until the promised harvest of facts is reaped and garnered. Until then, when theory and discussion will be on firmer ground, may the gathering of the harvest prosper!

CAMBRIDGE, MASS.

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# THE FOSSA PHARYNGEA IN AMERICAN INDIAN CRANIA

By LOUIS R. SULLIVAN

THE fossa pharyngea, fovea bursae, or medio-basial fossa is a small oval depression in the ventral surface of the basilar part of the occipital bone. The major axis lies in the antero-posterior direction in the median line. It varies in depth from 2 millimeters to 7 millimeters. The width is approximately 4 millimeters on the average while the length varies from 5 to 11 millimeters.

The function or purpose of the fossa is not altogether clear. Anatomical text-books dismiss it with a sentence. Thompson[1] writing in Cunningham says: "An oval pit, the fovea bursae or pharyngeal fossa, is sometimes seen in front of the tuberculum pharyngeum. This marks the site of the bursa pharyngea. . . . The origin and morphological significance of this pouch are not yet solved." Romiti[2] and Agostino[3] claim that the fossa pharyngea is produced by a pharyngeal diverticulum either abnormal or accessory. This is in agreement with the opinion stated above. Perna[4] concludes that the fossa pharyngea can be explained as a survival of that part of the median basilar canal which passes below the perichondrium on the ventral surface of the basilar portion of the occipital bone. The basilar part of the occipital bone ossifies like a vertebra and the fossa is the result of the non-ossification of the hypochordal bow element due to the position of the notochordal element in this region. I am not in a position to state the relative merits of the two opinions nor am I altogether certain that they are necessarily contradictory.

[1] Arthur Thompson and David Waterson in *Cunningham's Text-book of Anatomy*. New York, 1917.

[2] Romiti, 1891.

[3] Agostino, 1901.

[4] Perna, 1906.



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However, its anthropological importance and utility are not wholly dependent on its physiological, morphological, or phylogenetic significance, but in great part on its relative frequency. According to all authors consulted the fossa pharyngea is a rather uncommon structure both in man and other animals. Perna[1] and Agostino[2] give very little data on its frequency. Romiti[3] found it five times in 700 crania (0.7%). He quotes Gruber as finding it 46 times in 4000 to 5000 skulls or in about 1 percent of the cases examined. Le Double[4] records its frequency as 1.4 percent on the basis of 5000 skulls examined. Rossi[5] is the only author to my knowledge who has attempted to segregate his material racially. His results follow:

In 2911 European crania the fossa occurred 33 times or 1.31 %.
In 801 non-European crania the fossa occurred 31 times or 3.87 %.
In 240 Papuasian crania the fossa occurred 10 times or 4.16 %.
In 159 Asiatic crania the fossa occurred 7 times or 4.40 %.

The only conclusion one may draw from the above data is that the fossa pharyngea is of relatively rare occurrence and certainly

TABLE I

GROUPS REPRESENTED BY AT LEAST TWENTY CRANIA WHICH SHOW A RELATIVELY HIGH FREQUENCY OF THE PHARYNGEAL FOSSA

| Group | Number of Skulls[6] | Number with Pharyngeal Fossa | Percent with Pharyngeal Fossa |
|---|---|---|---|
| "Basket Maker" Utah, Grand Gulch | 97 | 26 | 26.8 |
| Cora Indian Mexico | 21 | 5 | 23.8 |
| California Indian (Hrdlička)[7] | 42 | 7 | 16.6 |
| Huichol Indian, Mexico | 32 | 5 | 15.5 |
| Utah, Grand Gulch, deformed | 22 | 3 | 13.6 |
| Tlanepantla, Mexico | 23 | 2 | 8.6 |
| San Simon, Mexico | 49 | 4 | 8.2 |
| Tarahumare, Mexico | 48 | 3 | 6.2 |
| Total | 334 | 55 | 16.4 |

[1] Perna, 1906.

[2] Agostino, 1901.

[3] Romiti, 1891.

[4] Le Double, 1903.

[5] Rossi, 1891.

[6] Only crania or calvaria having the basilar part of the occipital bone in good condition are included in the totals throughout.

[7] Hrdlička, 1906.

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not frequent enough to be of any great significance racially. It appears to be somewhat more frequent in Asiatic crania than in European crania. But even here the material is grouped in such a way that its significance is obscured. Bearing in mind the tremendous differentiation of mankind at the present time, material studied under such headings as European, Asiatic, and Papuasian can throw very little light upon our modern anthropological problems. Especially is this true in the study of such characters as the fossa pharyngea and other anomalous conditions. There is

TABLE II

GROUPS REPRESENTED BY A SMALL NUMBER OF CRANIA BUT SHOWING A RELATIVELY HIGH FREQUENCY OF THE PHARYNGEAL FOSSA

| Group | Number of Skulls | Number with Pharyngeal Fossa | Percent with Pharyngeal Fossa |
|---|---|---|---|
| Papago | 1 | 1 | 100.0 |
| Clear Creek, Arizona | 6 | 2 | 33.3 |
| Tepecano, Mexico | 4 | 1 | 25.0 |
| Guatemala Indian | 6 | 1 | 16.6 |
| Williamson County, Tennessee | 8 | 1 | 12.5 |
| Illinois | 8 | 1 | 12.5 |
| Miscellaneous Plains Indian | 10 | 1 | 10.0 |
| Otomi, Mexico | 11 | 1 | 9.0 |
| Total | 54 | 9 | 18.5 |

TABLE III

GROUPS REPRESENTED BY A LARGE NUMBER OF CRANIA WHICH SHOW A LOW FREQUENCY OF THE PHARYNGEAL FOSSA

| Group | Number of Skulls | Number with Pharyngeal Fossa | Percent with Pharyngeal Fossa |
|---|---|---|---|
| Tarascan, Mexico | 130 | 7 | 5.4 |
| Hank O'Kala, Huata, Bolivia | 17 | 1 | 5.3 |
| Salish, Washington | 24 | 1 | 4.3 |
| City of Mexico | 25 | 1 | 4.0 |
| Chinook | 92 | 3 | 3.3 |
| May's Lick, Kentucky | 45 | 1 | 2.2 |
| Kwakiutl | 87 | 2 | 2.2 |
| Eastern Eskimo | 50 | 1 | 2.0 |
| Takana Chullpa, Bolivia | 50 | 1 | 2.0 |
| Peru, Coastal Region | 58 | 1 | 1.7 |
| Charassani, Bolivia | 144 | 2 | 1.4 |
| Tama Tam Chullpa, Bolivia | 184 | 2 | 1.0 |
| Kupa Pukeo Chullpa, Bolivia | 144 | 1 | .7 |
| Total | 1050 | 24 | 2.3 |

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every reason to believe that these characters develop in individuals and are transmitted by inheritance. Their local distribution is of much greater significance than their racial distribution.

TABLE IV

GROUPS IN WHICH NO PHARYNGEAL FOSSAE WERE FOUND

| Group | Number of Crania Examined |
|---|---|
| Chuckchi, Siberia | 7 |
| Eskimo, Indian Point, Siberia | 32 |
| Eskimo, St. Lawrence Island, Bering Strait | 13 |
| Eskimo, Alaska (Point Barrow) | 102 |
| Athapascan, Alaska | 8 |
| Tsimshian | 9 |
| Haida | 40 |
| Yakima | 15 |
| Salish, Eburne, British Columbia | 54 |
| Salish, Kamloops | 13 |
| Salish, Nanaimo | 13 |
| Salish, Thompson | 26 |
| Salish, Bella Coola | 20 |
| Salish, Saanich | 14 |
| Salish, Lillooet | 8 |
| Kwakiutl, Nimkish | 41 |
| Kwakiutl, Nootka | 23 |
| Kwakiutl, Bella Bella | 10 |
| Washington State, Miscellaneous | 15 |
| Oregon State | 27 |
| California | 21 |
| New Mexico, Pueblos | 86 |
| Arizona, Chaco Cañon | 22 |
| Colorado | 6 |
| New York State | 33 |
| Massachusetts and Connecticut | 6 |
| Ohio (Madisonville) | 17 |
| Michigan (Saginaw) | 7 |
| Virginia | 1 |
| Georgia | 1 |
| North Carolina | 1 |
| Florida | 25 |
| Mexico, Yaqui | 7 |
| Mexico, Casas Grandes | 11 |
| Mexico, Zacateco | 4 |
| Columbia, Santa Marta | 10 |
| Peru, Maranon Country | 20 |
| Peru, vicinity of Cuzco | 18 |
| Peru, Sillustani | 11 |
| Peru, Cachilaya | 34 |

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TABLE IV.—*Continued.*

GROUPS IN WHICH NO PHARYNGEAL FOSSAE WERE FOUND

| Group | Number of Crania Examined |
|---|---|
| Isle of Titicaca, Bolivia | 64 |
| Isle of Cojato, Bolivia | 2 |
| Bolivia, Chujun Paki | 95 |
| Bolivia, Lluchini Amaya | 18 |
| Bolivia, Belen Chullpa | 8 |
| Bolivia, Churkoni Chullpa | 55 |
| Bolivia, Tiahuanaco | 3 |
| Cape Horn, | 3 |
| Total | 1079 |

TABLE V

TOTAL FREQUENCY OF FOSSA PHARNYGEA IN AMERICAN CRANIA

| Total Crania Examined | Total Crania with Fossa | Per cent of Crania with Fossa |
|---|---|---|
| 2517 | 88 | 3.5 |

I have not much non-American material at my disposal but the small amount available suggests that the fossa pharyngea occurs with a much greater frequency in some areas than in others. Out of five crania from New Hebrides two have the fossa pharyngea well marked. Two out of four crania from the Solomon islands have it also. The number of cases are too small to permit any valid conclusions but suggest a high frequency. In a series of thirty Hindu crania and forty Bedouin-Samaritan crania the fossa pharyngea was not found.

Turning to the American material I first became interested in the fossa pharyngea during the study of a group of Basket-maker crania from Grand Gulch, Utah. About twenty-five percent of the crania examined showed a larger or smaller fossa pharyngea. A little later I encountered it again in some Mexican Indian crania. Hrdlička[1] also found it in his examination of California Indian crania.

I then decided to examine all the Indian and Eskimo crania in the collections of the American Museum of Natural History. The results are tabulated in Tables I to V. Considering the crania as a whole the fossa pharyngea is not of very frequent occurrence in the American Indian and Eskimo. Of the 2517 crania examined it was present in 88 of them or 3.5 percent of the cases. This per-

[1] Hrdlička, 1906.

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centage is somewhat lower than that found by Rossi in miscellaneous collections of Asiatic and Papuasian crania.

When we consider the frequency in local groups the distribution becomes significant. All of the groups of high frequency in Table I are in the southwestern United States and Mexico. However it was not found in the crania of the pueblo peoples of New Mexico, Arizona, or Colorado. The distribution follows quite closely the distribution of the linguistic stocks tentatively grouped together as Uto-Aztecan. The crania in which it occurred with greatest frequency were moderately elongated with a cranial index averaging about 76 to 78. There is some overlapping in the distribution, notably in the case of the Otomi and Tarascan groups. This could undoubtedly be explained by contact and intermixture. On the other hand the fossa was not found among the Yaqui or Zacateco crania. In a few cases our material is inadequate to serve as a basis for valid deductions. This is true especially in the case of the Papago, Clear Creek, Arizona, and Otomi material. While our material as a whole may be taken as a fair sample in many areas, it is particularly deficient in the Plains area, Southeastern area, Plateau area, and in eastern and southern South America.

On the basis of the material at hand it seems that the frequent occurrency of the fossa pharyngea is limited to that area of North America which is or was the home of the Uto-Aztecan linguistic stock. The Basket-makers belong undoubtedly to the physical type which predominates in the Uto-Aztecan speaking peoples. It seems fair to assume that they also spoke some related dialect. From the data at hand we can only speak of southern affinities. In order to determine northern relationships it would be necessary to examine Plateau and Plains material. Such material is not at present at my disposal. The point I wish to make is that such characters as the fossa pharyngea have a similar distribution to that of the cephalic index, stature, etc., and are of equal value in determining the relationship of local groups. I hope that other collections of American crania may be studied for the presence or absence of the fossa pharyngea.

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# THE CULTURAL AND SOMATIC CORRELATIONS OF UTO-AZTECAN

By P. E. GODDARD

MODERN anthropology has one practically undisputed dogma. Culture being independent of biological inheritance is a thing separate from race or a particular biological strain. Culture, of course, includes language. In the middle of the nineteenth century the linguistic students in Europe used to speak of an Aryan race as well as Aryan languages. Toward the end of that century nothing could be more heretical than to speak with so little discrimination. This lesson of the separateness of language and race was exceedingly important in Europe and equally so in America.

Dr. Wissler[1] has recently reopened the subject by pointing out that actual conditions in North America do show a correlation between physical types, language, and other elements of culture. The Eskimo have skulls sufficiently peculiar to be easily distinguished, a culture of their own, and a language shared in only by the Alieut. There are also the Caribou-hunting, Athapascan-speaking peoples of the Mackenzie drainage.

On *a priori* grounds there ought to be some correlation between physical type, language, and culture. It is true that the body itself is derived by a peculiar biological mechanism, and that this has little or no influence on language or the variety or type of culture. Language and culture however are normally derived largely from the parents and grandparents, the same individuals from whom physical peculiarities are inherited.

If we imagine a valley of ample extent, the walls of which constitute definite barriers, occupied by a single family, a hundred generations should produce a definite biological strain of consider-

[1] *The American Indian*, chap. XIX, pp. 327–341.



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able uniformity, a common language, and a common general culture.

It is perhaps permissible to consider this the normal, expected condition. When such correlations do not exist interferring conditions are to be sought. An African race speaking English in North America is certainly unusual enough to require an explanation.

If a Siouan language is found near the south Atlantic coast of North America, one quite naturally speaks of a movement of peoples from the Plains region which carried the language southeastward. It is fairly proper to attempt a reconstruction of the distribution of the population in America on the basis of a correlation of language and physical type bearing in mind however that this correlation does not depend upon a casual relation. One of the main difficulties in such attempts at reconstruction has been the intangibility of the characterizations of the physical strains. Even when to an experienced eye the biological differences in tribes can be recognized, it has been difficult to reduce these observations to a statement or formula. The Eskimo are an exception. Prevailing longheadedness also is sufficiently uncommon to differentiate an occasional group.

Particularly interesting has been the differentiation in southern Utah of two types of culture. One of these is known as Cliff-dweller and the other Basket-maker. The former built many-roomed stone houses, and made several varieties of pottery. The latter did not build permanent houses, and made little pottery. That the basket-making peoples did not acquire the house-building, pottery-making habits, was demonstrated when it was observed that basket burials were always associated with the remains of people with unusually long heads which were in no instance deformed. The stone house-building people practised the flattening of the back of the head. Even if the heads had not been shortened artificially they are prevailingly less long than those of the basket-making people.

Now these basket-making people had certain elements of culture which connect them with the far south. They are in fact the

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northern representation of a broad general culture which formerly extended southward to Chili. The important common elements may be named as the raising of maize, grinding it on a metate, the raising of cotton and weaving it and other vegetable fibers on a simple frame loom, the wearing of sandals, and the use of the spear thrower. The culture of maize originated far south of Utah. It is probable agriculture as well as the other arts mentioned above reached Utah by transmission from one people to another.

We have recently learned that the building of many-roomed houses of stone and adobe, and the manufacture of corrugated and paint-decorated pottery are arts which arose in the Southwest itself. While we have no conclusive evidence, it seems probable that the more highly developed forms of present day Pueblo religion, the dramas and processions, were also developed in the Southwest. Some of the simple foundation elements of the religion however extend beyond the Pueblo region. Of these may be mentioned feathered offerings, color symbolism, the use of cornmeal for sprinkling, etc.

Powell, actuated by a commendable scientific caution, separated the languages of the southern plateaus, the Shoshonean, from the Piman of southern Arizona and northern Mexico, and from the Nahuatl of the valley of Mexico. Kroeber[1] in 1907 reunited these groups under the name of Uto-Aztekan. We have then a large linguistic group stretching from southern Mexico nearly to the Canadian boundary.

While these Uto-Aztecan-speaking peoples cannot be said to have occupied a valley similar to the hypothetical one mentioned at the beginning of this article, they did occupy a series of such valleys, extending from north to south. The Rocky mountains and the high Sierra of Mexico formed a barrier on the east. The Sierra Nevada and the Mohave desert were a partial barrier on the west. Unlike our hypothetical valley, the Uto-Aztecan range was so great that instead of linguistic uniformity we find three great linguistic subdivisions. The Shoshonean proper may be imagined

[1] "Shoshonean Dialects of California," *University of California Publications in Archaeology and Ethnology*, vol. IV, pp. 66–156. Berkeley, 1907.

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as having been originally north of the Colorado river.[1] From the Gila southward, west of the Sierra Madre are the Piman peoples; south and east of these mountains are the Nahuatl of Mexico valley. These linguistic groups were evidently in existence in the sixteenth century, and the changes in the languages since they were first known have been slight. For the development of such linguistic groups milleniums should be allowed.

As oil follows up a wick, so the culture of the south may be imagined to have passed northward among this people of a common speech. It went north along this particular line as far as cotton and maize can be grown. Had the culture followed the Pacific coast it might have reached Oregon at least.

We seem then to have a fairly definite correlation between language and a culture which has no necessary relation to language. Have we a further correlation with a definite physical type? The longheadedness of the Basket-makers, the Pima, and Papago, and certain Mexican tribes has indicated such a special type. The paper by Mr. Sullivan on the "Fossa Pharyngea" establishes a definite physical group having a common line of inheritance. The very insignificance of this fossa precludes any argument for its occurrence as a result of environment. The backbone of southwestern physical, linguistic, and cultural early distribution seems to have been established. It remains to trace similar relations and connections for the groups east and west of this Uto-Aztecan speaking, maize and cotton raising, longheaded people who in uncommon numbers, have a fossa pharyngea. With a physical type definitely correlated with Shoshonean speech it ought to be possible to determine whether any considerable number of such a physical strain were incorporated in the Shoshonean-speaking Hopi villages. If a definite physical type can be established for the stone house building peoples of the San Juan drainage it should be possible to trace the movement of the people themselves as well as their culture. This is especially promising since types of pottery of definite geographical and temporal sequence have been established.

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[1] The Comanche in Texas and the Paiute in southern California have been assumed to have left the plateaus in fairly recent times.

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## WHO WERE THE PADOUCA?

BY GEORGE BIRD GRINNELL

ACCOUNTS of early exploration in the West seem to show that the Padouca, under their various similar names, occupied the central plains from the Black Hills region south to the Arkansas or beyond. The name was familiar for one hundred years, and then passed out of use. It appears on early maps, but not in a way to indicate much about the tribe's relationship to other peoples. Some of these maps are mentioned here:

D'Anville's map, said to be drawn in 1732, published in 1755, has the Kansas river marked "R. des Padoucas et Kansez."

du Pratz's map of Louisiana, 1758, shows Padouca villages in four places:

1. On north bank of Arkansas river about northeast of Santa Fe.
2. On the head of a southern branch of the Kansas river—possibly the Smoky Hill. This village is marked "Gr [and] Village of the Padoucas."
3. On the n. bank of the n. fork of Kansas river [(?) Republican river] about due north of the Grand village.
4. On a southern branch of the Missouri, which probably is intended for the Platte.

He says the distance from the Padouca village to the Kanzes village on the Missouri is 65½ leagues. The length of a French league is uncertain, but it was not more than 2.75 miles. This would make the distance perhaps 180 miles or less and would put the Grand Padouca village about as far above the forks of the Kansas river as Ellsworth, Kansas.

This map shows no Apaches at all in the plains east of New Mexico and north of Texas.

Palairet's map, 1755, marks Kansas river, "R. Padoucas." Sets down village of the Padoucas as situated 232 miles west of the



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junction of Kansas river with Missouri river and on the 40th degree of north latitude. This would be on the Republican river.

The old Spanish map brought back by Lewis and Clark, and printed in the Atlas Volume VII of Original Journals shows little circles indicating fixed villages on the north bank of the Platte, and marked "Padouca Inds." This map was probably made late in the eighteenth century, perhaps 1790 to 1800. I know of no record of whites visiting the Padouca on the Platte, but the traders from St. Louis were among the Pawnees from about 1750 onward at irregular intervals.

Mallet, in 1739, applied to one of the forks of the Platte the name *Padocas river* and on the Spanish map just referred to, the south fork of the Platte is marked *Padouca Fork*, and James in Long follows this.

The locations given for the Padouca on the French maps after 1720 seem to show that they were in the heart of the plains country, reaching from south of the Canadian north across the head of that stream, across the Arkansas, the Kansas river, to and well beyond the Platte. The French maps sometimes divide these people into white and black Padouca just as they divided the Pawnees into white and black.

Omaha and Ponka traditions speak of a Padouca village in the sand hills near the Niobrara river or the head of the Elkhorn. It was by a lake and is so shown on the du Lac map, but the village was abandoned long before his day.

There was a Padouca village on the Dismal river north of the Platte river, which was visited by Ponkas during their wanderings on the plains at an early date. This village is mentioned in Fletcher and La Flesche.[1] It is possible, but not likley, that this may be the same Padouca village shown on du Lac's map. The country near the head of the Dismal river formerly had many lakes, some dry and some with water. There are many lakes in that part of Nebraska.

The Omaha call the Dismal river "Where the Padouca built breastworks."

[1] *Twenty-seventh Annual Report, Bureau of American Ethnology*, 1911, "The Omaha," p. 88.

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The Caddoan people, visited in 1719 near the junction of the two forks of the Canadian, spoke to La Harpe and du Tisne of Padouca villages on the Arkansas, the Cimarron, and the Canadian, east of the Spanish settlements.

We have always been taught that the Padouca were Comanche, but, in fact, were they so?

The term, so far as I know, is not used by the Spaniards, who were more likely to speak of *los gentiles Cumanches*, as in documents quoted by Bandelier.[1] Bandelier says that between the years 1700 and 1705 the Utes brought the Comanches first down into New Mexico at the pueblo of Taos. Up to this time the Utes and different bands of Apaches are the only *gentiles*—wild Indians—mentioned in the plains to the north and east of the New Mexico frontier. The Spanish documents quoted by Bandelier in the same paper seem to show that there were Apaches living toward the plains east of the most northerly Spanish settlements. In 1719, the Apaches Jicarillas lived at a place called Jicarilla about thirty miles from Taos and about one hundred miles from Santa Fe, somewhere near the head of the Canadian river. Bancroft (*History of New Mexico*, p. 239) says that in 1724 this settlement was attacked by Comanches who carried off half the women and children and killed all the rest of the inhabitants. The Apache village called Quartelejo was one hundred forty miles north by east of Jicarilla. About 1748, the remnant of these Apaches was driven by the Comanche to take shelter at the Taos and Pecos pueblos.

Bandelier says that Quartelejo was about three hundred miles northeast of Santa Fe and believes it was in western Kansas near the southern line of Nebraska. The documents state that the distance was carefully reckoned—*demarcado*. Every Spanish expedition had with it a man who carefully reckoned each day's march and set down the distance. The site of Quartelejo is thought to have been identified recently—in Scott county, Kansas.

In 1719, Don Antonio Valverde Cossio, Governor in New Mexico, made an expedition to Quartelejo and there heard of those Pananas who lived on a great river seventy leagues north of Quar-

[1] *Papers of the Archaeological Institute of America*, vol. v, p. 184.

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telejo, and that among the Pananas were some Frenchmen. Panana was the Spanish name for Pawnees, who had long been known to the New Mexico Spaniards, and is the name today applied to the Pawnees by all the Pueblo Indians, including the Zunis. In March 1720, Colonel Villazur was ordered to go to the Pananas and, on his way, to establish a presidio or garrison at Quartelejo. It was later decided that a presidio could not be built at Quartelejo because it was so distant in the barren plains and because it had so little wood and good water. The Apaches, who lived there, left the place in winter, because of the lack of wood, and wintered elsewhere.

Villazur set out with a force of fifty Spaniards, armed with guns, and had with him a force of Indians constantly referred to in the documents of the time as Apaches Jicarillas. Incidentally, he took with him, besides servants, a mule loaded with his own table silverware, which later, certain narrators referred to as "church plate." The expedition set out June 14, 1720, and on September 6, Tamariz, a soldier, reached Santa Fe with the news that the command had been destroyed.

A Pawnee slave among the Apaches had been taken along by Villazur as guide and, after sixty-two days' march from Santa Fe, the company reached a large river August 15th. There was a Panana village on the north bank of the river and Villazur sent his Pawnee man to the village to speak to the people; but the man did not return. Villazur became alarmed and retreated one day south and camped on a river and in the night advanced again and camped on a southern branch of the river where the Pawnees were. The next morning he was attacked with "volleys of musketry and arrows." His Apaches fled and presently such of the Spaniards as were left alive also ran off. Forty Spaniards, including Villazur and—probably—the priest, were killed, and about ten escaped and reached Taos. The Spaniards declared that there were Frenchmen with the Pawnees. This story is well known and has often been told.

Under date of July 20, 1721, Bienville, then Governor of Louisiana, wrote a letter declaring that two hundred Spaniards and a



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large force of Padoucas had marched toward the Missouri to attack the French in the Illinois country; but they were attacked by the Otoes and the Panimaha, who killed all but one Spaniard and many Padoucas. This one Spaniard, he said, was a prisoner among the Indians, and de Boisbriant, commandant in the Illinois, had sent for him. (Margry, vol. VI, p. 386).

In this Bienville was only repeating what de Boisbriant had written him April 24, 1721. It is to be noted that these Frenchmen said that the massacre was on the Kansas river, while Bandelier and Dunbar believe it was on the Platte.

The interesting point as to all this is that the Spaniards declared that the Indians who accompanied Villazur were Apaches, while the French, on information from tribes of the Missouri, referred to these native allies of the Spaniards as Padoucas.

Bourgmont, in 1724, left the Kansas village on the west bank of the Missouri north of the Kansas river and marched west or northwest for ten days to reach the Padouca village. A part of his people were on foot, and part mounted. He writes quite fully of the country he passed over and what he says of it suggests that the small camps he passed and this large Padouca village may have been somewhere near the junction of the Saline and the Smoky Hill rivers. He says these people hunt in summer and winter but are not entirely wandering—are partially sedentary—for they have villages with large houses (*cabanes*), and do some planting. The nation is very numerous and extends for about two hundred leagues. Those distant from the Spanish settlements used flint knives to skin their game and flint axes to cut down trees. The village visited had about one hundred and fifty houses, and the population is given as eight hundred warriors, fifteen hundred women, two thousand children, or nearly thirty persons to a house. This implies permanent houses like those of the earth-lodge people of the Missouri River valley. Into an ordinary skin lodge or tipi not more than ten or fifteen people could be crowded. Thus it would seem that the Padoucas must have lived in villages similar to those of the Pawnees and other earth-lodge people. The Comanches are not supposed to have had permanent houses, or to have

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planted. Stories told by the Pawnees within forty years declare definitely that the Comanches knew nothing of the growing of crops.

Spanish documents quoted by Bandelier say that the Apaches of New Mexico in the early part of the eighteenth century lived somewhat in the same way as did the Padouca described by Bourgmont. They had houses, jacales, and huts where they planted in spring, but they did not live at these villages the year through.

It is believed that during the eighteenth century, almost from its first years, the Comanches began to make raids on the people of the plains and especially on the Apaches who finally were driven in from some of their settlements close to the Pueblo villages.

The Apaches were never mentioned by the French, Apache being a southwestern term, the use of which seems never to have extended into French territory. The French hardly refer to any tribes on the plains or on the New Mexican frontier except the Padouca, until the time of Mallet, 1739, when the French first reached New Mexico and found "Laitanes" on the New Mexican frontier. On the other hand, the Spaniards seem to mention no large tribes between the Apache, who occupied the country beyond the New Mexican frontier, and the tribes near the Missouri river. They speak of the Apache as extending out from the New Mexican borders and beyond them to the east were the Panana, Jumano, and other Caddoan tribes; and to the northeast beyond the Apache were other Pananas who were the Pawnees of the Platte.

It must be remembered that scarcely any original observations were made by Lewis and Clark away from the Missouri river. They saw something of the Sioux and something of the village tribes, but for information about the people of the plains, were obliged to depend on the Indians and the few white men who had been long in the country.

For this reason in cases where they report only statements at second hand, the accounts of early explorers generally must not be taken too literally. On the other hand, those who saw the Indians about whom they wrote no doubt reported with substantial accuracy on matters which came under their own observation.

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I give below certain mentions of the Padouca by early writers:

Pénicaut, 1698–1722, B. F. in French, *Annals of Louisiana*, New Series, 1869, p. 152, notes disappearance of Padouca who claimed a wide country according to the Delisle map—1712—but are supposed to have broken up into Wetepahatoes, Kiawas, and Kattekas. Lewis and Clark obviously took this from Pénicaut.

P. 155, mentions Heitans.

Mallet Brothers 1739–40, Margry, vol. VI, p. 455. They seem to speak of Ietan and Padouca as distinct. Of the Ietans—Laitanes—it is said that they wander in the country north of Taos, and are not Christians. A camp met by the Mallet brothers had a Ricara captive. After the Mallets had started east, they met a camp of Laitanes on the head of a branch of the Arkansas, possibly the Canadian. Further east in the plains, they met two men and three women of the Padouca, who became frightened and ran away.

Du Lac 1802, Paris and Lyon, 1805; pp. 225–26; a nation dwelling on the Missouri; Peducas, 300; furnish about fifty skins to the upper Louisiana traders. This list includes tribes near, as well as those on, the Missouri river and this band of Padouca or Cataka may be the one spoken of by Lewis and Clark as sometimes visiting Rees and Mandans.

P. 261: "The great nation of the Padaws who range along the Platte river is only about ten days' ride from the Ricaras," then estimated to number 25,000. This is the same French nickname given for the Padoucas by Lewis and Clark. The description would seem to place their villages about on the heads of the Loup river. Immediately afterward he mentions the wandering Baldheads or Halisanes, and tells where they roam—on the Arkansas and west to the mountains of New Mexico.

P. 309: Tells how the Otoes on their summer hunt were attacked by a war party of Halitanes.

These references suggest that du Lac considers Halitanes, Halisanes, Baldheads, and Tetes Pelees as different from the Padaw. The first named are the Comanche. Like this, the information given Lewis and Clark and du Lac, seems to show that the Padaw

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or Padoo were not Comanche, that they were supposed to inhabit the country near the Upper Platte, and that some of them at times traded at the Missouri.

Trudeau, 1795, *Missouri Historical Society Collections*, vol. IV, p. 31, describes the Pados almost exactly as du Lac has done and seems also to locate them on the heads of the Loup river. La Salle, in 1680, when on the Illinois river was visited by Indians from the west and they spoke to him about the Gattahka and other tribes among whom were the Pasos. Were these perhaps the Pados?

*Lewis and Clark Original Journals*, vol. I, p. 190, gives information obtained at Ree villages, 1804. In the list of tribes that live on the plains to the west of the Rees one is given Cat-tar-kah, interpreted as Paducar. This information was presumably had through a French interpreter, for the other tribal names in the list are translated into English. This would seem to show that the French on the Upper Missouri considered the Cataka to be Padouca.

Volume VI contains a table of tribes made up at the Mandan villages in the winter of 1804–5, and so probably came from Mandan, Hidatsa, and French information.

P. 90: Cataka—a tribe that occasionally come to the Mandan villages to trade as do also the Cheyennes, Kiowas, Arapahoes, Staetans, and Crows.

P. 101: Cat a kah, their own name; Ha ka?, given thus with a question as French name for these people; 300 people; traders do not visit them; they at times visit the Ree villages. This Cataka tribe roves with the Dotame, and Nemousin, from the head of the Loup Fork north to the heads of the southern branches of the Yellowstone. "One of these tribes is known to speak the Padoucan language."

P. 102: Dotame—do-ta-ma, their own name; no French name given. They speak the Padouces language; 120 people; no traders visit them; they never come to the villages on the Missouri; raised many horses.

P. 102: Nemousin, the other band said to rove with the Cataka-ni-mi-ou-sin, own name; no French nickname; 200 people; never

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visit the villages on the Missouri; the Handbook identifies these people as Comanche.

P. 106: Alitan or Snake Indians—French nickname Gens du Serpent, speak Alitan language. Very numerous; have many horses; all tribes on Missouri war on them; there are three divisions: 1. Aliatan, Snakes, or Só-so-na, trade with Crows and have some trade with Spaniards; 2. Those of the West who live west of the mountains; 3. La Playes occupy plains from head of Arkansas south across the Red river.

P. 108: Padoucas—English name, French nickname Padoo, Padoucies is their own tongue. Live in villages on heads of Platte and Arkansas, trade with New Mexico; many horses. Yet almost immediately Clark says he could get no definite information about this once powerful nation, and quotes French writers. Speaks of a fork of the Platte bearing the name of the tribe and conjectures that the nation had broken up and become individual small tribes. This from Pénicaut.

All this including the Key to the Tribes used in Indian Statistics, vol. v, p. 82, shows that from Lewis and Clark we learn little more than this: that in 1804 the one tribe well known at the upper Missouri villages who were called Padouca were the Cataka.

Pike's Exploration made in 1806. Coues Edition. Coues points out that Pike's Tetaus is a misprint or corruption from Ietans, which is evident. On the map at the end of the third volume is indicated Ietan country. Other plains tribes villages are indicated on Kansas, Platte, and Loup rivers.

P. 535: the Tetaus who occupy the heads of Red and Arkansas rivers, the Canadian, and the Arkansas—and extend to the Del Norte—and the Utahs and Kyawas who live in the "mountains of North Mexico," are the enemies of the Pawnee.

P. 536: "The Tetaus, Camanches, as the Spanish termed them [called] Padoucas by the Pawnees,"—the word "called" is inserted in brackets. This information was given Pike by the French interpreter and seems to show beyond question that at this date the French on the lower Missouri identified the Padouca as Comanche. I do not suppose that the Pawnee generally used the term

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Padouca. They had their own name for the Comanches, *La ri ta*, from which possibly might be derived La li tanes, Laitanes, and perhaps Alitans, which however Mooney derives from Ietan.

P. 536: The Tetaus are a powerful nation, wandering, do not plant; bounded on south by tribes of Lower Red river, on west by Spanish settlements, on east by Osages, Pawnees, and others, and on north by Utahs, Kyaways, and unknown tribes.

P. 591: Statistical Table says Tetan, English name; Comanche, Indian name; ——, French name; 8200 people. Then he says, the Osages made war on the Tetaus, on several other tribes, and on the Padoucas. This points to the Padouca as distinct from the Tetau and others.

James in Long's Expedition, Philadelphia, 1823. Mentions the Padouca in only two places.

The Pawnees, through a French interpreter, described the tribes with which the Skidi war party had a big battle south of the Arkansas in the winter of 1819–20, as Ietans, Arrapahoes, and Kiawas.

If Padouca was the name used by the Pawnees generally for the Comanche, as Pike states, why did not the Pawnee and Oto speak of them by that name to James in 1820, instead of using the term Ietan?

We have, in 1802, du Lac speaking of Halisanes, Halitanes, Tete Pelees, and Baldheads; Lewis and Clark, 1804, Alitans, or La Plays; and James, 1820, Ietan, or La Plais, "Bald Heads, and a few Shoshone or Snake Indians," and in another place "Ietans, Comanche, or Snakes."

In 1820 there was a famous Oto chief named Ietan who got his name from his exploits against that tribe. If Padouca was the Oto and Pawnee name for the Comanche, why was not this man called Padouca instead of Ietan?

Journal of Jacob Fowler, 1821–22. In November 1821, Glenn and Fowler's party met Kiowas, and two or three days later 350 lodges of Highatans. These people held a council and demanded the presents which Major Long had promised them when he had met them in 1820. Fowler on counting the increasing camp found

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400 lodges of the following bands: Ietans, Arrapohoes, Kiawa-Padduce, Cheans, Snakes; the Ietans most numerous. This seems to suggest that the Kiawa-Padduce were Kiawa-Apaches, for the people whom Fowler calls Kiawa-Padduce in one place and in another place Padduca, were with the Kiowa and were not Comanche. Yet Glenn's interpreter, Roy, was a man from the Osage country, and in 1853 Neighbors says Padduca was the Osage name for the Comanche or Ietan.

James' *Three Years Among the Indians and Mexicans*, Mo. Hist. Soc. 1916; does not use the name Padouca nor Ietan, but speaks of the Comanche. In one place he speaks of an Ietan chief with a big party of Pawnees met on the Arkansas June 18, 1822. This is probably the same party of which Fowler speaks, Pawnees and one Ietan chief, but Fowler says that this Ietan chief had recently been to Washington; and it seems probable that he was the Oto chief named Ietan, and not a Comanche at all. James quotes a letter in the Louisiana Gazette, a Missouri newspaper printed in 1810, where is mentioned the "Aytan, or Padoco Nation."

W. P. Clark, *Indian Sign Language*, p. 33. W. P. Clark, about 1880, met a Kiowa-Apache about seventy years old who told him that he had been born on the Missouri river northeast of the Black hills, a statement which indicates that as late as 1810 the Kiowa-Apaches were still in the country where the Cataka had roamed in the days of Lewis and Clark.

All this seems to suggest the probability that in early days the people known as Padouca were not Comanche. If the Comanche had then occupied the central plains, where the French place the Padouca, the Spaniards would have known of it and would have recorded the fact. That the Utes first brought the Comanche to Taos, and that these two tribes were associated for some years thereafter, and that about 1730–40 the Comanche are known to have occupied the country from Taos to and beyond the Arkansas, justifies, to my mind, the belief that the Comanche came South near to the mountains.

Our later information about them tends to strengthen this suggestion. Ruxton, p. 254, speaks of a tradition that the Comanche

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and Shoshoni separated on Fountain Creek north of Pueblo in the edge of the mountains. Pike, Lalande, Pursley, Chouteau and De Mun, James, and other early sources, all mention the Comanche and "Snakes" as using trails in the edge of the mountains or among the mountains on their frequent journeys from the Arkansas to the Platte. One of the very early mentions of the Comanche out in the plains is in Bandelier where it is stated that in 1744, thirty-three Frenchmen visited the Comanche on the Rio Jicarilla at the head of the Canadian, east of Taos, and traded them guns. A few years later, we read in the Texas annals that the Comanche are pressing down to Red river, forcing the Lipans southward into Texas.

The mention by La Salle's men of a tribe in Texas, whose name they spell Caumuche, has been thought to show that the Comanche were in Texas in 1680; but it is a mere guess, as I suppose, that this name has anything to do with our name of Comanche.

The tribe spoken of as the Kiawa-Padduca by Jacob Fowler in 1821 may well have been Kiawa-Apache, and the name points to the survival of the use of the name Padouca for the Apache as late as 1821.

What has been written shows that the Spanish records of the expedition under Villazur in 1721 called the Indian allies Jicarilla-Apaches, while the French account called these same Indians Padouca.

Bourgmont says that the Padouca had fixed villages with large houses and that they planted; and the early Lewis and Clark map records fixed villages for that people. On the other hand, the books and the Pawnee Indians declare that the Comanches did not raise crops, and had no fixed villages.

Du Lac implies that the Padaws and the Halitanes were different tribes.

Lewis and Clark say that the Cataka were Padouca and that the Snakes, Alitan, La Plais, were different from the Cataka.

Pike says that Padouca was the Pawnee name for the Comanche. He also says that the Tetaus or Tetan—Ietan—were Comanche. Then he says that the Osages made war on the Tetans and on the

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Padouca, thus implying that these are different tribes. He speaks also of the Tetaus as different from the Utahs which is evidence that Mooney's conclusion that the Ietans are Utes is not well founded—but Mooney does not say that the Ietans of later days were Utes—in fact, the books generally imply that the Ietans are Comanches.

Jacob Fowler found, with the Kiowa, some people whom he called Kiowa-Padduce and Padduca—people who were not Comanche.

W. P. Clark, about 1880, met a Kiowa-Apache about seventy years old, who said he was born on the Missouri river northeast of the Black hills.

The evidence is not conclusive as to who were the Padouca, but it convinces me that the Padouca were not Comanche and I am disposed to regard them as Apache.

NEW YORK CITY

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# NASS RIVER TERMS OF RELATIONSHIP

By E. SAPIR

THE following Nass River Indian (*Nɪsqa''ᵃ*) terms of relationship were obtained in May, 1916, from Chief C. B. Barton (Indian name *P'ä·l*), of Kincolith, B. C. Chief Barton was at the time engaged as deputy in Ottawa on tribal business.[1] The orthography here employed is the same as that explained in my *Sketch of the Social Organization of the Nass River Indians* (Geological Survey of Canada, Anthropological Series, Bulletin no. 7, 1915); see pp. 29, 30.

## I. Nass River Terms

In the following table it is to be understood that, unless otherwise indicated, a term may be used by either a male or a female. Most or all of the terms doubtless have a wider, phratric or clan, significance than is here indicated. The ending -ɪ'(ʲ), -ɛ'(ʲ), -'ʲ is the first person singular possessive suffix, "my."

Note further:

1. Step-relations are designated as real relations.

2. The parents of a married couple are not looked upon as relatives. This is strikingly different from the custom of many western American Indian tribes, among whom there is frequently a specific term for "child-in-law's parent."

3. The parent-in-law of a brother or sister is not considered a

[1] Since this set of terms was obtained from Mr. Barton, an opportunity has presented itself in April, 1920, to go over the data with two West Coast Indians visiting Ottawa on Government business—Mr. P. C. Calder, a Nass River Indian of the *Ɣɑɬxat'ɛ'ⁿ* tribe, from the village of Greenville, and Mr. G. Matheson, a Tsimshian Indian, who was brought up among the Nass River Indians and is thus better acquainted with the Nass than with his own dialect and who has for a number of years resided in the Lower Fraser country. As both of these Indians are also conversant with the Tsimshian dialect, I obtained from them an independent set of Tsimshian kinship terms. This set supplements a Tsimshian set obtained in 1918 from Mr. W. Beynon, of Port Simpson, B. C., Mr. Barbeau's chief Tsimshian interpreter. I shall present my Tsimshian data in another paper.



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| Term | Translation | Vocative |
|---|---|---|
| 1. *'o·''ls-ɪ'* | my great-grandfather, great-grandmother (see also 2 and 3) | *'o·''ls*[3] |
| 2. *nɪyɛ''-ɛ'*[1] | my grandfather (paternal *or* maternal); may also be used for great-grandfather; grandparent's brother | *yɛ''*[3] |
| 3. *ɳɬ'sɛ·''ɪ's-ɪ'* | my grandmother (paternal *or* maternal); may also be used for great-grandmother; grandparent's sister | *tsi·''ts* (note unglottalized *ts*, doubtless due to imitation of simplified children's pronunciation)[3] |
| 4. *hoxdä·'k'ɛn-ɛ'* | my grandchild; great-grandchild | *hoxdä·'k'ɪn'* |
| 5. *nɔgwɔ·''d-ɪ'* | my father; father's brother; maternal aunt's husband | *pä·p'* (said by man) *hädɪ·''*[1] (said by woman)[3] |
| 6. *nω·''-*[1] | my mother; mother's sister | *nä·''ä*[3] |
| 7. *łko·''ᵘłgw-ɪ'*, plur. *łg-ɪ'* | my child; man's brother's child; woman's sister's child; husband's brother's child; man's brother's wife's child; wife's sister's child; *probably also* woman's sister's husband's child | *łko·''ᵘłkᵘ*, plur. *łgɪ'* |
| 7a. *łko·''ᵘłkvm gä·'ᵃd-ɪ'* | my son (*lit.*: my male child) | *nä·''äɪ'*[3] |
| 7b. *łko·''ᵘłkvm änä'g-aɪ''*[1] | my daughter (*lit*: my female child) | *dä·ɬ*[3] |
| 8. *nɔpɛ'p-ɪ'* | my mother's brother | *pɪ·p'* |
| 9. *nɪxdä·'-'*[1] | my father's sister; mother's brother's wife | *dä·t'* |
| 10. *kwvclɪ'c-ɪ'* | my brother's child (woman speaking); sister's child (man speaking); husband's sister's child[2] | *kwvclɪ'c* |
| 10a. *kwvclɪ'cɳ gä'ᵃd-ɪ'* | my brother's son (woman speaking), sister's son (man speaking) (*lit.*: my male *kwvclɪ'c*) | |
| 10b. *kwvclɪ'cɳ änä''-aɪ''*[1] | my brother's daughter (woman speaking), sister's daughter (man speaking) (*lit.*: my female *kwvclɪ'c*) | |
| 11. *wä'g-ɪ''*[1] | my brother (man speaking); man's father's brother's son; mother's sister's son | *wäk'* |
| 12. *łgɪ·''gw-ɪ'* | my sister (woman speaking); woman's father's brother's daughter; mother's sister's daughter | *łgɪ·''ᵘkᵘ* |

[1] Also heard as *änä''aɪ''*[1] (' = glottal stop with velar resonance). In Nootka ɛ develops regularly from older Wakashan *q'*.

[2] "My husband's sister's child" was given as *kwvclɪ'ckw-ɪ'*, but this is almost certainly merely the plural in *-kᵘ* of *kwvclɪ'c-ɪ'* and should thus be understood as "my brother's children (woman speaking), my husband's sister's children." See F. Boas, *Tsimshian*, §44 ("Handbook of American Indian Languages," *Bulletin 40, Bureau of American Ethnology*, 1911).

[3] See supplementary notes at end.

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| Term | Translation | Vocative |
|---|---|---|
| 13. *kɪmxɬɪ'-'ɪ*..... | my brother (woman speaking), sister (man speaking); woman's father's brother's son, mother's sister's son; man's mother's sister's daughter, father's brother's daughter | *kɪmxɬɪ'ᶜ* |
| 14. *kwɯɬx̣a'ω''-'ɪ*..... | my cross cousin, *i.e.*, father's sister's child, mother's brother's child | *'ω''ʷs* |
| 15. *nä'kc-ɪ'*..... | my husband; wife | *näkc*[3] |
| 16. *łä'mc-ɪ'*..... | father-in-law, mother-in-law; son-in-law, daughter-in-law; father-in-law's brother, mother-in-law's brother | *łämc* |
| 16a. *łä'mcɪm gä''ᵃd-ɪ'* . | my father-in-law, son-in-law, parent-in-law's brother (*lit.*: male parent-in-law *or* child-in-law) | |
| 16b. *łä'mcɪm änä''-aɪ''ɪ*..... | my mother-in-law, daughter-in-law (*lit.*: female parent-in-law *or* child-in-law) | |
| 17. *q'aɬä''ᵃn-ɪ'*..... | my wife's brother; sister's husband (man speaking) | *q'aɬä''ᵃn* |
| 18. *kwvdjɪ''c-ɪ'*..... | my husband's sister; brother's wife (woman speaking) | *kwvdjɪ''c* |
| 19. *k'ω''ɬ'kc-ɪ'*..... | my wife's sister, man's brother's wife; husband's brother, woman's sister's husband | *k'ω'ɬ'kc* |
| 20. *łämcɬ wä''ᵃg-ɪ'*.... | my brother's child-in-law (man speaking) (*lit.*: child-in-law of my [man's] brother) | |
| 21. *łämcɬ (ɬ)gɪ''ɪ-gw-ɪ'*..... | my sister's child-in-law (woman speaking) (*lit.*: child-in-law of my [woman's] sister) | |
| 22. *łämcɬ gɪmxdɪ'-'ɪ* .. | my [man's] sister's child-in-law; woman's brother's child-in-law (*lit.*: child-in-law of my sibling[1] of opposite sex) | |
| 23. *gɪmxdɬ łä'mc-ɪ'* .. | my father-in-law's sister (*lit.*: sister of my father-in-law[2]) | |
| 24. *lgɪ''ɪkᵘɬ łä'mc-ɪ'* .. | my mother-in-law's sister (*lit.*: sister of my mother-in-law) | |
| 25. *nɔ x̣ᵃ-ɪ'*..... | my father's brother's wife | *nɔx̣*[3] |
| 26. *näkcɬ nɪxdä''-'ɪ* .. | my father's sister's husband (*lit.*: husband of my paternal aunt) | |
| 27. *kwvclɪ'ckᵘɬ nä'kc-ɪ'* ..... | my wife's brother's children (*lit.*: brother's children (see 10.) of my wife) | |
| 28. *wɪlä''ɪckᵘ*; *wɪlä''ɪcgw-ɪ'*..... | relative; my blood relative[3] | |
| 29. *nvwvlwɪlä''ɪckᵘ* .. | relative by marriage[3] | |

[1] By "sibling" is meant "brother or sister."

[2] But not "brother of my mother-in-law," though this is quite possible etymologically. For "mother-in-law's brother," *łämc* is used (see 16).

[3] See supplementary notes at end.

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relative; nor, reciprocally, is the child-in-law's brother or sister a relative.

4. Relatives by affinity continue to be called by the same terms after the death of the connecting link. Thus, a man's brother-in-law (wife's brother) is termed *q'alä·'ᵃn* even after his wife's death. This again is contrary to the custom of many western American Indian tribes.

## II. Linguistic Comments

A few linguistic remarks are possible, though, for the most part, the terms do not yield to any far-reaching linguistic analysis. Most striking is the employment of distinctive vocatives. In most cases (nos. 1, 4, 7, 10, 11, 12, 13, 16, 17, 18, 19, 25) the vocative is merely the noun stem, unprovided with a possessive suffix. In a considerable number of cases, however, the vocative is different from the noun stem. Sometimes the vocative is etymologically unrelated to it (nos. 5, 7a, 7b, perhaps also 14), more often it is a shorter or otherwise modified form of the stem (nos. 2, 3, 6, 8, 9). A number of nouns beginning with *nɔ-* (*nɪ-*, *n̥-*) lose this element in the vocative (nos. 2, 3, 8, 9). It is probable that this prefix occurs also in the term for "father" (no. 5); possibly also in that for "mother" (no. 6).

The etymology of the *n-* prefix is quite obscure, as there seem to be no obvious analogies in the formative elements of either Nass or Tsimshian proper ascertained by Boas. It may be an old classificatory prefix for terms of relationship, now preserved only in four or five terms. Possibly, however, it is the subjective first person singular pronominal prefix *n-* "I" (*e.g.*, *ne·-ya''ne·* "I say so," contrast *de·-ya* "he says so"; see Boas, *op. cit.*, §53), originally characterizing, it may be, terms of relationship as contrasted with other nouns. In that case such a form as *nɪ-yᵉ''ᵉ* "grandfather" would originally have meant "my grandfather," only secondarily, as the use of the *n*-prefix in a possessive pronominal sense became obsolete, "grandfather." The use of the first personal singular possessive pronominal suffix *-ɪ'* in such terms of relationship would be due to the analogy of the vast majority of nouns. At any rate,

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analogous pronominal usages, of an isolated nature, for terms of relationship are found in several American Indian languages.[1]

The terms *kwvclɪ'c* (no. 10), *kwvtxa'ω'* (no. 14), and *kwvdji'c* (no. 18) possess a prefixed element *kwv-*, as is clearly shown by the corresponding Tsimshian terms *sle's*, *txaɔ'* and *dzu's*.[2] The prefix is not listed by Boas in his grammar, but it seems not unlikely that it is identical with the *gu-* of *gulɪkˇs-* "backward; also reflexive object,"[3] the second element of which can hardly be other than the prefix *ləks-* "strange, different, by itself."[4] This analysis makes it at least possible that *kwv-* is a reciprocal prefix: "each other." The terms *kwvtaxˇ'ω'* "cross-cousin" and *kwvdji'c* "woman's sister-in-law" are, indeed, directly reciprocal terms; while *kwvclɪ'c* "child of sibling of opposite sex to speaker," though not strictly a reciprocal term, does involve what might be called "sex reciprocity" between the speaker and the connecting link.

It is barely possible that *kwv-txa'ω'* (no. 14) contains, besides, the common prefix *txa-* "entirely, all,"[5] and that the stem proper is *-'ω'*. If this is so, the term may originally have been a reciprocal collective: "all cousins to one another." A stem *-'ω'* would make the vocative *'ω'ʷs* appear somewhat less enigmatic.

*nvwvlwɪlä'ʷɪckʷ* (no. 29) is obviously a reduplicated form of *wɪlä'ʷɪckʷ* (no. 28), preceded by a prefixed element *nv-*, which is either a phonetically weakened form of reciprocal *na-* "each other, one another,"[6] or another example of the relationship prefix *nə-* already discussed.

The vocative *dä't'* (no. 9) shows a reduplicated form of the stem analogous to *pɪ'p'* (no. 8).

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[1] The hypothesis here advanced seems fairly unplausible from the purely Tsimshian standpoint. I hope, at some future time, to adduce certain comparative linguistic evidence that serves materially to strengthen it.

[2] See F. Boas, "Tsimshian Mythology," *Thirty-first Annual Report, Bureau of American Ethnology*, 1916, p. 493. Boas' Tsimshian orthography is modified to correspond to my own.

[3] *Op. cit.*, p. 323, no. 115.

[4] *Op. cit.*, p. 327, no. 133.

[5] *Op. cit.*, p. 318, no. 93.

[6] *Op. cit.*, p. 319, no. 95.

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## III. Discussion of Terms

1. A considerable proportion of the terms are indifferently used as regards the sex of the person designated (nos. 1, 4, 7, 10, 14, 15, 16, 20, 21, 22, 27, 28, 29). Others explicitly refer to the sex of the person designated (nos. 2, 3, 5, 6, 8, 9, 11, 12, 17, 18, 23, 24, 25, 26). In a few cases the sex is fixed in opposition to the sex of the speaker (nos. 13, 19). In nos. 7a and b, 10a and b, and 16a and b the explicit sex reference is a purely secondary feature.

2. In a considerable number of cases the sex of the speaker is taken account of. These are: nos. 5 (in vocative), 10, 11, 12, 13 (conditionally), 17, 18, 19 (conditionally), 20, 21, 22 (conditionally), 27.

3. The sex of a connecting relative or of connecting relatives is considered in nos. 5 ("father's brother"), 6 ("mother's sister"), 7 ("child of sibling of same sex as speaker"), 8, 9, 10 (conditionally), 11 ("nephew, niece"), 12 ("nephew, niece"), 13 ("nephew, niece"), 14, 17 (conditionally), 18 (conditionally), 19 (conditionally), 20 (conditionally), 21 (conditionally), 22 (conditionally), 23, 24, 25, 26, 27 (conditionally). It is particularly noteworthy that the sex of the connecting relative (father's or son's generation) does not count in nos. 2, 3, and 4.

4. Reciprocity is illustrated in nos. 11, 12, 13, 14, 15, 16, 17, 18, 19, 28, 29. Not counting the last two terms, which are hardly relationship terms proper, it will be noticed that all these reciprocal terms, except *lämc* (no. 16), are terms of the same generation. Reciprocity in nomenclature does not obtain, as it so often does in America, between grandparents and grandchildren, nor between uncle-aunts and nephew-nieces.

5. The distribution of the terms for "uncle" and "aunt" and, reciprocally, for "nephew" and "niece" is conditioned by whether or not the siblings of the older generation are of the same sex. If they are, the "uncle" is merged with "father" (no. 5), the "aunt" with "mother" (no. 6), the "nephew" or "niece" with "child" (no. 7). If not, special terms are used, "paternal aunt" (no. 9), "maternal uncle" (no. 8), and "cross-sibling's[1] child"

[1] "Cross-sibling" means "woman's brother" or "man's sister."

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(no. 10). A natural consequence of this distribution of terms is the classification of cousins into "siblings" (nos. 11, 12, 13) and "cross cousins" (no. 14). Whether these facts are explainable on the basis of the exogamic phratry organization of the Tsimshian tribes, as would be currently assumed, or of the levirate, is not clear. Perhaps neither factor is the historically primary cause.

6. The distribution of terms for "uncle's *or* aunt's spouse" and, reciprocally, for "spouse's niece *or* nephew" is somewhat curious. The maternal aunt's husband is classed with the father (no. 5); reciprocally, the wife's sister's child with one's own child (no. 7). The paternal aunt's husband is designated by a descriptive term, "husband of paternal aunt" (no. 26), to which corresponds, as reciprocal, a descriptive term, "cross-sibling's-child of wife" (no. 27). The maternal uncle's wife is classed with the paternal aunt (no. 9); reciprocally, the husband's sister's child is classed with one's cross-sibling's child (no. 10). Finally, the paternal uncle's wife is classed with the mother (no. 25); the corresponding reciprocal term for the husband's brother's child is classed with one's own child (no. 7).

7. Somewhat unexpected is the distribution of terms for "sibling's child-in-law" and, reciprocally, for "parent-in-law's sibling." The child-in-law of the brother or sister is consistently designated by purely descriptive terms (nos. 20, 21, 22). The reciprocals, however, are only partly analogous. The sister of the parent-in-law is descriptively defined (nos. 23, 24), but the brother of the parent-in-law is merged with the parent-in-law (no. 16).

8. The fairly extended use of transparent descriptive terms (cf. English "father-in-law") is noteworthy (nos. 20, 21, 22, 23, 24, 26, 27). Analogous formations occur further south in Washo and in certain Shoshonean systems. That these terms are to be looked upon as genuine terms of relationship, not merely as formations *ad hoc*, seems to be indicated by the fact that their range of actual significance is more restricted than that of their etymological significance (see note to no. 23; also supplementary note 3, p. 261). Thus *g̱ɪmxdɪl lä'mc-ɪ'* "my father-in-law's sister" (no. 23) has a far wider etymological significance, as it might also refer to "my



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son-in-law's sister, daughter-in-law's brother, mother-in-law's brother." Of these three relationships, however, the first two fall outside the circle of recognized affinity, while the third is merged with the parent-in-law.

Summarizing the most striking peculiarities of the Nass River system of terms of relationship, we may point out that it:

1. Makes considerable, but by no means exhaustive, use of the principles of reciprocity and of sex differences in speaker, person designated, and connecting link.

2. Rather frequently merges lineal with collateral kindred.

3. Confuses, to at least some extent, relations of affinity with relations of consanguinity.

4. Makes some use of transparently descriptive terms.

5. At no point recognizes the principle of seniority which is all but universal in aboriginal America (*e.g.*, "older brother" and "younger brother").

6. Possesses a number of distinctive vocative terms.

## IV. Supplementary Notes Due to Mr. P. C. Calder

*To 1.* Neither Mr. Calder nor Mr. Matheson seemed very familiar with the term *'o·''ls*. "Grandfather" and "grandmother" are generally used instead. Mr. Calder has heard *'o·''lsɪ'* used among the *Gɪtlaxt'ä·'mɪkc* band, further up the river, and is inclined to think that the term was originally confined to the upper villages, there having been old dialectic differences among the Nass River people that are now ironed out. This hardly seems likely in view of of the fact that the term *'o·''ls* was easily remembered by Chief Barton, of Kincolith, which is at the very mouth of the river, and has been obtained for Tsimshian proper by Dr. Boas (through Nahum Tate) and by Mr. Beynon. When the Tsimshian terms were obtained, Mr. Matheson remembered hearing *'o·''ʷls* used in his childhood for "great-grandmother," but was not certain whether it also applied to "great-grandfather." Mr. Calder claimed that if it was necessary to distinguish the "great-grandparent" from the "grandparent," it could be done by referring to the latter as "my great grandfather" (*'wi·t'e'sɪm niyɛ''e'ⁱ*) or "my great grandmother"

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('*wi·t'e'sɪm ṇt'se·''t'sɪ*'). It is difficult to believe that these paraphrases are anything but modern imitations of the English terms, though Mr. Calder claimed they were old Nass River usages.

*To 2, 3.* Mr. Calder claimed that in the old days the maternal and paternal grandparents were distinguished, but he does not remember how this was done.

*To 5.* The term *pä·p*ˑ can also be used with the possessive suffix: *pä·'bɪ*' "my father" (male speaking), but only as a vocative, not as a term of reference. The term *hädɪ·''*ⁱ evidently has the first person singular possessive suffix. Mr. Calder fancied this term was derived from *hä·t*ˑ, the word for "intestines," but this is simply an example of folk-etymology. Mr. Matheson gave similar folk-etymologies for the Tsimshian terms for "grandfather" and "grandmother," which he has learned from an old Tsimshian. Both of these Indians claimed that the older members of their tribes knew the "real" meanings, *i.e.*, the supposed etymologies, of all the kinship terms. The existence of such folk-etymologies for kinship terms is itself an interesting fact. The probable etymology of *hädɪ·''*ⁱ has been suggested to me by Miss Theresa Mayer. The fact that the same non-vocative form for "father" is used by both males and females in Nass River and Tsimshian (Dr. Boas states that Tsimshian *a·b* is used by women only for "father," but this is incorrect; *ä·'bo* "my father" is an obsolescent term indicating great respect and used by both sexes) and, further, the fact that the Tsimshian vocative does not seem to distinguish the sex of the speaker make it likely that the Nass River usage is a secondary one. The word *hädɪ·''*ⁱ cannot be explained by reference to anything else in Nass River or Tsimshian. It is altogether likely that it is simply borrowed from the Haida vocative *ha·da·'i*, used by a female child in addressing its father. This term is evidently simplified from the regular vocative, *χa·da·'i*, of Haida *χa·t-ga*, *χa·d-*, the term for "father of female." The Haida differentiation of "father" according to the sex of the child applies to both vocative and non-vocative forms. This would be but one of several facts tending to show that the Haida had closer cultural relations with the Nass River people than with the Tsimshian proper. The Nass

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River problem is complicated by the existence of a phonetically similar term in Upper Lillooet: *hä'tɛ* "father" (vocative only, apparently for both sexes).

*To 6.* This term includes also the "father's brother's wife." No. 25 rests on a misunderstanding. The term *nɔ'x̣ᵃi'* (*nɔ'x̣ᵊi'*) is simply a diminutive or endearing form of the more formal *nω·''ᵢ*. This *nɔ'x̣ᵃi'* "my little mother" is not only used endearingly for the mother, mother's sister, and father's brother's wife, but also, by a curious reciprocal usage, for the child or grandchild of an affectionate mother or grandmother. In the latter sense it can only be used by a female.

*To 7a, 7b.* The term *nä·''ä't'* is also used in a wider sense. It may be employed by any man or woman in addressing a male to express great regard and affection. The term *dä·l* is used analogously, except that it may be employed by a woman only. There are also two terms of reference, not used as vocative or with possessive suffixes, for "boy, son" and "girl, daughter." From birth up to the time that he is given a name, the son of a family is referred to as *gɪnɛ·''ᵊs;* from birth up to the time that the first mark is made on her lower lip for the eventual insertion of a labret, the daughter is known as *'ax̣q'e't's*, literally "without a labret mark." These terms are paralleled by the Tsimshian *gɪnɛ·''ᵊs* and *qa''ᵘs*.

*To 15.* According to Mr. Calder, *näkc* is not used as a vocative. The proper vocative usage for "spouse" is a teknonymous one. If the mother has a son or daughter young enough to be referred to as *gɪnɛ·''ᵊs* or *'ax̣q'e't's*, her husband will address her as *nɔx̣c gɪnɛ·''ᵊs* or *nɔx̣c 'ax̣q'e't's*, "mother of the boy!" or "mother of the girl!" Analogously, the wife will address her husband as *nəgwɔ·''ᵊt's gɪnɛ·''ᵊs* or *nəgwɔ·''ᵊt's 'ax̣q'e't's*, "father of the boy!" or "father of the girl!" When the spouses no longer have a son or daughter young enough to be referred to as *gɪnɛ·''ᵊs* or *'ax̣q'e't's*, they address each other as "father of so and so!" or "mother of so and so!", using the name of one or the other of their sons or daughters.

*To 25.* This term, as already noted, is simply a form of the word for "mother." See preceding note and note to 6.

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*To 28, 29.* These terms were misunderstood. No. 29 is merely a collective form of 28; its meaning is "relatives all together." The terms do not specifically refer to blood-relatives, but apply to all relations, whether by blood or marriage. If necessary, the blood-relative may be distinguished as *ləp-wɪlä·"ɪckᵘ*, "self-relative, relative *par excellence*."

GEOLOGICAL SURVEY,
OTTAWA, CANADA

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# GAMES OF THE CALIFORNIA INDIANS

By A. L. KROEBER

THE endless games of the North American Indians have been reduced by Stewart Culin to a few fundamental types:

| | | | |
|---|---|---|---|
| Games of Dexterity | With a dart | Outdoor | Hoop and pole |
| | | Hand | Ring and pin |
| | With a ball | | Shinny, lacrosse, etc. |
| Games of Chance | Pure chance | | Dice |
| | Guessing | | Hand or stick |

Among amateurs, the guessing games come out entirely according to luck; among skillful players they depend on concealment and reading of facial and bodily expression, and are therefore in reality games of mental ability, or rather of will and character.

As a rule, all of these games were known to all the California Indians, and in many cases each game existed in only one form or variety in the same locality. The exceptions are so few that it would not be a very great distortion of the truth to say that every group played only five and substantially the same five games.

The hoop and dart game was perhaps the only one which was entirely unknown in some districts. The Yurok and Hupa did not possess it, and it seems to have been lacking also through the remainder of the northwestern part of the state. It is an interesting circumstance, illustrating again that the northwestern corner of California is ethnographically the last frontier of the North Pacific coast, that this hoop-and-pole game, favorite over a large portion of the continent, also holds but small part in the amusements of most of the coast tribes from Oregon to Alaska.

The dignity of this game is upheld at the opposite end of the state, where the Mohave deem it the means of gambling best befitting a man. They play it with a small string-wound hoop, and long poles that are slid so as to fall, if possible, on or under the rolling hoop when this finally comes to rest. The Luiseño and Diegueño,



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the other tribes of the south, the Salinan and Costanoan groups, the Maidu; the Pomo, the Shasta, and the Modoc, followed substantially the same game. Among the Yokuts, Mono, and Miwok of the Sierra Nevada, youths and boys played a simpler and typically Californian variety. A small block was thrown or slid, and then poles darted after it.

The ring and pin or hand variety of the same game, in which several rings or loops are strung to the butt end of a peg on which they are to be caught, is widespread in California, but varies characteristically according to habits of life and, ultimately, environment. The salmon-fishing tribes of the northwest, as far south as the middle course of Eel river, including the Tolowa, Yurok, Hupa, Chimariko, Shasta, and Sinkyone, employed salmon vertebrae as "rings." On the headwaters of Eel river, where the streams run smaller and hunting largely replaces fishing, the Wailaki used deer bones. In the south, the Luiseño favored acorn cups; while the agricultural Mohave made their rings of pumpkin rind. The Klamath and Modoc employed a single-looped ball, made of the same tule rush that is the material of most of their industries. The Maidu and Yokuts did without this game, so far as known.

Of the many possible varieties of ball games, each group usually specialized on one. The Pomo played a kind of lacrosse, with a rude, small net. Still simpler rackets are found among the Southern Maidu. With the Miwok and Yokuts the net has degenerated into a mere loop at the end of a stick, serving to pick up or pocket the ball rather than bat it. Among both these groups this rudimentary form of the racket is perhaps due to the shinny stick being the standard form of ball-propelling implement. The Miwok women, but not the men, also batted a soft hair-stuffed ball with baskets resembling the utilitarian seed-beater.

The Mohave knew nothing of lacrosse, but clung to simple shinny, played with a small block or ball and plain curved sticks. With these they played as our boys play shinny or hockey on the ice.

It would have been difficult to find many suitable fields for such

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an active free-running game in the rocky canyons of the northwestern tribes; even the bars and river benches are narrow, rough, and uneven. Here, accordingly, the game was played with a double ball of two string-tied blocks of wood, impossible to propel far by striking, and requiring to be picked up with the end of the stick and thrown. Manoeuvering thus took the place of speed, the players grappled like wrestlers, and a number of men could participate within a small area.

Elsewhere than in the northwest, the double ball game is essentially or wholly one for women, as over most of the continent. This is the case among the Shasta, Modoc, Achomawi, Washo, Maidu, and Miwok. Among the last three groups the "ball" has degenerated into merely the connecting string, though this is heavy and sometimes knotted at the ends.

Through most of the south, and along the coast as far north at least as Monterey, sticks or bats were dispensed with, and the game became essentially a foot-ball race. The contestants covered a long distance, each hurling, with his feet only, his little wooden ball. Speed and endurance were counted as even more valuable factors toward victory than skill in manipulation. Diegueño, Luiseño, Costanoan, and presumably the intervening groups competed in this way, which was familiar also to the Indians of Arizona. The Chumash, however, knew shinny; and in the interior the ball race had penetrated to the Maidu and Miwok. This latter people followed all the varieties of ball play: rackets for men and for women, shinny, double ball for women, and foot ball race.

Dice were everywhere preëminently if not entirely a woman's game. A set numbered four, six, or eight, each only two-sided; the count of the various combinations of pieces falling face up or face down, varied locally. The Yurok, Tolowa, Wiyot, and Hupa used four mussel-shell disks; the Pomo, Wailaki, and Northern Yokuts, six split sticks; the Mohave, Diegueño, and Luiseño four painted boards; the Southern Yokuts, Chumash, and Chemehuevi, filled walnut shells. Among the Miwok, split acorns were employed, and among the Mono, acorn cups. The Modoc used either the Californian sticks, or a northern type consisting of four beaver teeth. Some tribes played on a flat basket, others on a stone.

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The ball game, whatever its character, was well fitted for competition between towns or districts, and was often heavily backed with stakes; but, except with the Mohave and perhaps the Yokuts,—who favored respectively the rolling hoop and the shinny stick,—the gambling game above all others, and therefore the man's game par excellence, among the California tribes, was the "hand" or "grass" game, a contest of guessing. Tremendous energy and concentration were thrown into this play, which was passionately followed. Songs and sometimes drumming were regular features, without which the stimulus to play hard would be weakened, and the contestants' luck magically diminished. Actually, the singing and rhythmic swaying aided the player to conceal his knowledge of the location of the "ace" by enabling him better to control his expression.

A public ritual, a dance, even a mourning ceremony, could hardly take place without the accompaniment, at least at the conclusion, of the guessing game. It is hard for us to realize to the full the large degree to which this amusement—or occupation—entered into the life not so much of a professional class of gamblers as of all the California Indians. Their avarice, and the importance to them of their wealth, hardly allowed them to bet as recklessly, and to strip themselves as completely of all belongings on a run of ill luck, as some of the eastern tribes, with whom liberality rather than possession carried prestige; but they made up in the frequency, the duration, and the tenacity of their play.

Two types of the game occur, and these do not differ fundamentally. In the northwest, a bundle of twenty-five to fifty slender rods is used, one being painted in the center. These sticks are shuffled in sight of the opponent with a peculiar rolling twist, divided behind the back, and then shown, the middle portions concealed in the hands. After some deliberation, and frequent false or pretended starts, the opponent guesses for the hand containing the one marked stick, indicating his decision by pointing past the other hand. If he is right, he wins nothing but the privilege of playing; if wrong, one counter goes to the player, who shuffles again. An expert player always knows the place of the marked

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rod among its many plain fellows, even behind his back, and frequently displays it alone against the pack in his other hand, to tempt his opponent to incline to the latter; or, divining the tendency of his mind, misleads him with a single unmarked rod.

The Shasta, the Northern Wintun, and the Modoc play like the northwestern tribes; but through the remainder of the state, from the Wailaki and Achomawi to the Diegueño, the implements are two small bones, or short sticks, one of them marked with a band. These are concealed in the two hands behind the back, under a mat, or often wound in two wisps of grass in view of the opponent, whence the popular American name of the game. Some tribes use only one small bone, guessing for the full hand; mostly they employ four, handled by two men on a side; the southern Indians usually attach string loops to pass over the fingers; but such differences do not seriously alter the course of the play.

The counters are everywhere sticks. Contrary to our custom, the Indians rarely begin with an equal number of markers on the two sides, but with a neutral pile from which winnings are allotted to this or that contestant. Only after this stock is exhausted, do they begin to win from each other; and the game continues until one side is without sticks. This may be an affair of minutes. But if fortunes are fluctuating and ability even, one contest may be prolonged for hours. If the losers, without a word, continue to play, they are understood to bet in the ensuing game an amount the equal of that which was staked by both parties in the first game. At least such is the Hupa custom.

Among the Mohave alone, so far as known, several varieties of the guessing game are played. One of these, shared by them with some of the tribes of the Southwest, is a smaller, informal affair pertaining to idle moments. A bit of stick is hidden in one of four little hillocks of sand. Dexterity of manipulation and perception seems the deciding factor rather than control of the features.

The Coast Miwok, some of the Maidu, and the Washo, played the regular "hand" game, but also guessed whether the number of a handful of manipulated sticks was odd or even. Among the Pomo the sticks were counted off by fours and the remainder

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guessed at. This procedure is suggestive of a Chinese form of gambling, but the geographical compactness of the area over which this sub-type of guessing game is found suggests its distribution from a native origin.

The "four-stick game" is another local variety, which has been found only among the Lutuami, Achomawi, Northern Paiute, and Washo—all at least partly Californian—and possibly the Chinook of Oregon. Among most or all of these tribes it does not replace but occurs by the side of the usual guessing game. Two of the sticks are heavy, two short and thin. The guessing is for the order in which they are grouped under a basket or mat.

When one reflects that in reality chance is no greater factor in the standard forms of the guessing game than in the American national card game, the decisive element being the match of character against character, the fascination which the game exercises on the Indian's mind is easy to understand.

The economic basis of life and the estimation of the purpose of wealth among the Indians are so different from our own, that gambling, instead of incurring odium, was not only sanctioned but approved. Nevertheless the underlying human similarity of the emotional processes connected with the practice is revealed in a most interesting way by the common belief in a connection between success at play and in relations with the opposite sex: lucky in love, the reverse at cards, and *vice versa*, is our proverbial superstition. But the Indian, regarding, like the ancient Hebrews and ourselves, sexual affairs as normally destructive of supernatural or magical potency, draws in a particular case an opposite inference. Two Yokuts myths relate how the favorite hero of these tales, the prairie-falcon, was uniformly successful in winning all stakes, in the one case at shinny, in the other with the hoop and poles, until the coyote was induced to disguise himself as the victor and thus take advantage of the latter's wife. As soon as this misfortune, although unknown, befell the falcon, his luck turned, until he lost everything. The modern gambler would perhaps expect the opposite event.

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# BOOK REVIEWS

## METHODS AND PRINCIPLES

*Primitive Society.* ROBERT H. LOWIE. Boni and Liveright: New York, 1920. VIII, 463 pp,

Dr. Lowie and American ethnology are to be heartily congratulated on the appearance of this book which meets a long felt want. It is a contribution to sociology of great importance, for it gives us a comprehensive account of the characteristics of the ruder forms of human society by one who has himself, by work in the field, gained a clear view of the principles upon which they depend.

The book may be considered from two points of view. In the first place, it is a valuable record of the known facts concerning the different forms of social organization, marriage, kinship, rank, and government from different parts of the world. In the second place there is a definite point of view running through the whole in the light of which many problems of theoretical interest are discussed.

In the latter respect Dr. Lowie shows himself an adherent of the historical as opposed to what is often known as the evolutionary school of thought, and chooses Morgan's scheme of the evolution of human society as the special object of his criticism. On the vexed questions concerning the respective rôles of diffusion and convergence he takes a moderate position, one which does not bring him into open conflict with the prevailing dogma of the independence of American culture. Especially valuable on the theoretical side is his discussion of the priority of the family or sib.[1] He goes far towards proving that in America the sib is later than the family, or at least later than that form of social grouping which is often known to American ethnologists as the band, and gives much evidence to show that the sibless organizations have not passed through a stage in which they possessed sibs. On the kindred topic of the time-order of the different modes of descent of the sib, he adopts the view that the family has evolved, sometimes into the patrilineal, and sometimes into the matrilineal sib, and rejects the view that one has always preceded or succeeded the other.

I believe that I shall be paying the best compliment to Dr. Lowie's

[1] The "clan" of other writers.



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work if I devote the major part of this review to criticism. I will begin with some comments on the book as a record of facts. Considering the wide field which it covers the number of omissions and errors is very small. In Oceania attention must be drawn to the omission of any reference to the *eriam* and *kimta* of Bartle bay in New Guinea, an omission especially unfortunate because these institutions furnish by far the most characteristic examples of age-grades and sexual communism in this part of the world. The omission is evidently due to lack of acquaintance with Dr. Seligman's book on *The Melanesians of British New Guinea* and is important because it has contributed to the acceptance, though in qualified form, of Schurtz's mistaken view that age-grades and men's societies are closely related to one another. The connection between age-grades and sexual communism both in New Guinea and among the Masai, and the absence of any such connection in the case of the secret societies of Melanesia, together with many other differences between the two kinds of grouping, suggest that in spite of a superficial resemblance, age-grades and men's societies belong to widely different categories of social institution.

In the references to Africa the omissions are more numerous and there are several somewhat serious errors. The generalization of Hahn that gardening with the hoe is woman's distinctive occupation is said to hold good of Africa, whereas as a matter of fact, men and women hoe the fields together among most Bantu peoples as well as in West Africa, while among some peoples, such as the Lendu of Uganda, the men work in the fields and the women do not. The fact that the men do not take part in horticulture among the Baganda while they do so among the more agricultural Bantu suggests that the division of labor in which women undertake the duties of horticulture is a relatively late result of the influence of the pastoral element of the Bantu peoples. Again, the statement that vendettas are rare in Africa is contrary to the facts, for many examples are to be found in such a work as Post's *Afrikanische Jurisprudenz*.

Some of the accounts of the Masai are misleading. Thus, a statement on p. 390 suggests that the smiths have sibs peculiar to themselves, whereas with one exception they are members of the same sibs as the rest of the people. It is also said that there is no suggestion of any racial differences between the smiths and the general body of the Masai. This does not agree with the opinions of Johnston and Elliot, both of whom connect the smiths with the Andarobo on the ground of physical similarity, an opinion strengthened by native traditions. Peculiarities

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of the language of the smiths, especially in words concerning their trade, also point to a difference of origin.

In the more theoretical portions of the book no objection can be raised to the author's attitude towards the older and cruder views of Morgan and his followers concerning unilinear evolution. On the other hand, Dr. Lowie's views concerning the nature of the process of diffusion are open to criticism, and this matter is so important in relation to the attitude adopted towards the problem of the independence of American culture that it must be considered at some length. The criticism in this respect must centre round Dr. Lowie's use of the concept of borrowing. Throughout the book he continually speaks of the process by which one culture influences another as borrowing, and uses this term even when the influenced and influencing cultures are widely separated from one another. In several passages, too, he speaks of contact between widely separated peoples, thus implying an attitude towards diffusion which I have already criticised elsewhere[1] in a review of Dr. Wissler's book on *The American Indian.*

There is no doubt that certain elements of culture such as dances, songs and material objects, may be directly borrowed by one people from another, though even in these cases the process usually involves such modification of the transmitted object or institution as to make the term "borrowing" unsatisfactory. In the vast majority of the cases in which one culture influences another, even in its immediate vicinity, the process has a dynamic character which makes the concept of borrowing inadequate and misleading. Even if the term were suited to the transactions between neighboring peoples, it becomes wholly inappropriate when a culture is influenced by another coming from a distance. In his use of the term "borrowing," and in all his references to diffusion, Dr. Lowie fails to appreciate the most important features of the process by which the diffusion of culture acts as a stimulus to new developments, a process in which a new idea or a new technique sets up a change, the general direction of which, as well as many of its details, is determined by the nature of the incoming influence.

The limitation of the author's outlook which is produced by his dependence on only one, and that a relatively unimportant, mechanism of diffusion is well illustrated by his treatment of the relations between the sib-organizations of North America. Five different areas of distribution of this form of social organization are distinguished and the differences

[1] *Man*, vol. XIX, (1919) p. 75.

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between them described. On account of these differences Dr. Lowie concludes that they, or at least four of them, are the results of separate processes of evolution and arose independently of one another. Various reasons are given to show that the people of one area have not "borrowed" from another although such borrowing is supposed to have occurred within each area.

One who takes a more dynamic view of the mechanisms of diffusion will readily accept Dr. Lowie's facts and yet be not at all disturbed in his belief in historical connection. Even if the social beliefs and sentiments underlying the sib-organization were carried to America only by one immigrant influence, the differences which exist are just such as might be expected. Thus, elsewhere there is definite evidence that the dual organization is independent of the sib-organization, and if moieties were already present in certain parts of America and absent in others when what I may call the "sib-idea" was introduced, we should expect to find just such differences in the relative importance of the moiety as exists among the Indians of the East and those of the Northern Plains. Or, the differences might be produced, though less probably, by the later introduction of the dual principle into the two regions. Again, the fact that in the east the sibs take their names from animals, while in the Northern Plains the people use nicknames for their sibs, would simply mean that for some reason the connection of the sibs with animals which is so frequent elsewhere, and was probably a feature of the introduced culture, failed to take root in the Northern Plains and was replaced by a nomenclature of a different kind. Once again, the special kind of ceremonial importance of the sib of the Pueblo tribes is not unknown elsewhere, and its presence, though in a specialized form, would be due to some feature of the Pueblo environment which led to the survival and accentuation of this feature of the sib-idea in that region. Lastly, the "crest" which forms the distinctive feature of the sib-organization of British Columbia becomes intelligible as an example of the development of totemism into heraldry which has almost certainly taken place elsewhere. The differences on which Dr. Lowie lays so much stress are capable of explanation on the lines of historical connection even if the sib-idea reached America only once, but they would be still more readily explicable if the ideas were brought on more than one occasion, and perhaps from different directions, not only, for instance, by way of the Pacific but by a pre-Columbian movement across the Atlantic by way of the Canary islands.

It is well to point out that in one sense such a process as I have out-

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lined is one of independent evolution in different directions, but starting from one or more introduced influences. The question at issue is whether the independence has been complete or whether the basic similarity which underlies the points of difference does not depend on the presence of some common influence. The author's failure to appreciate the dynamic character of diffusion reveals itself also when he is dealing with similarities in place of differences. When features of American culture present points of close similarity with those of other parts of the world, these similarities are discounted because the view which seems to underlie the customs elsewhere is un-Indian. This is, however, a two-edged argument. The hypothesis of independent evolution rests upon the determination of customs or institutions either by the social or the geographical conditions of the locality in which the independent evolution takes place. Since Dr. Lowie discounts the importance of the geographical factor (see p. 129) it ought to be possible to show that the independent evolution has in each case been determined by social conditions. The similar custom elsewhere must also have been determined, however, by its social environment. If, therefore, there is the difference in social atmosphere which Dr. Lowie indicates by the attribute "un-Indian," he will have to explain why two different social atmospheres should have produced the similarities with which he is dealing. He has to explain, for instance, why the highly specialized connection of animal names with sibs should have arisen in the very different social atmospheres of America, Oceania, India, and Africa. One who takes the more dynamic view of the process of diffusion regards the presence of these different atmospheres, not as arguments against diffusion, but as a means of explaining why a certain set of beliefs and sentiments spreading over the earth produces customs and institutions which, while preserving a basic similarity, yet differ greatly in the nature of their details and in the other features of culture with which they are associated.

The fault which underlies the whole of Dr. Lowie's treatment of the spread of culture depends on his adoption of a far too mechanical and "simpliste" view of the process of diffusion, a view crystallized in the term "borrowing." This is responsible for his adherence to the dogma of the independence of American culture to which he clings bravely in spite of many admissions which would surely have opened his eyes if his outlook had not been obscured by his unduly simple view of the mechanism of diffusion.

In considering this subject it will be useful to begin with some of these admissions. Dr. Lowie tells us that he was not impressed by the

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presence of the bullroarer in America, Australia, and Africa so long as the object itself was alone concerned, but when he finds that in all three places the object is used as a means of excluding women from ceremonial, he does not hesitate, in the light of our present knowledge, to accept diffusion as more probable than independent origin. In another place he remarks that it cannot be an accident that all the suggestions of the sexual dichotomy of society in North America are reported from the Pacific coast, this feature of the relation of the sexes being characteristic of the cultures which lie on the other side of the Pacific ocean. Again, he points out several resemblances (to which others could readily be added) in the institution of chieftainship in Polynesia and among the Indians of the Northwest, and only rejects the significance of the resemblance because of the general difference of character of the two cultures, an example of the argument which has just been considered. These examples show very clearly how Dr. Lowie has been influenced by the limitation of his outlook to the process of borrowing. His concessions are such as might possibly come into the category of borrowing, but do not impress one who takes the more dynamic view so much as the basic similarity between so many of the customs and institutions of North-America and those of other parts of the world. To one who takes the wider view these cumulative similarities are far more impressive than the wide distribution of a special kind of noise to frighten women.

I hope that my failure to deal sympathetically with one feature of Dr. Lowie's book will not be allowed to obscure my great appreciation of it as a whole. The views which I have criticised form only a minor feature of a book which will be of the greatest value to students as a record of early forms of social institution. If it is not greedy to ask for more where so much has already been given, I should like to express the wish that Dr. Lowie will give us another book dealing, in the same manner but more fully, with the facts and problems of the sociology of aboriginal America. It is a great merit of the present book that it supplements the invaluable work of Dr. Clark Wissler on *The American Indian* where that book is weakest. Dr. Lowie's book contains many instances of American sociology which the student of other regions of the earth would find it difficult to extract from the vast mass of material collected by the industry of American ethnologists. If Dr. Lowie would give us another book on American society, dealing, for instance, with the problems raised by the nomenclature of relationship and giving in more detail the evidence bearing on the time-order of different forms of social organization, he would confer a great boon on all those interested in thel history of social institutions.

W. H. R. RIVERS



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*The Social Evolution of Religion.* GEORGE WILLIS COOKE. Boston, The Stratford Co., 1920. 416 pp.

Durkheim developed with brilliancy the thesis that religion is a social product, integrated with the social life. The application of this was limited, in his exposition, almost entirely to Australian life.

Cooke may be said to start where Durkheim left off. He shows us the application of this theory to the larger field of the religious life.

There is an excellent statement of Durkheim's view and an application of it to much of primitive ceremonial—initiation ceremonies, for example. Out of these may have come the mysteries. There is a discussion of myth and an insistence that the application of the theory 'ritual first myth later' has been overworked. There are examples of the social transmission of human experience, as also of the creative genius of social man. Into this social order the individual injects his impulses and concepts, sometimes with success, sometimes to no purpose.

If the religious life is an integral portion of the social life it follows that we can not have change or advance in the one without accompanying changes in the other; the religious will, moreover, in some sense keep step with the social, if it is to retain a place in the social scheme. This consideration doubtless suggested the order of the last six chapters, which deal respectively with Communal and Tribal Religion, Feudal Religion, National Religion, International Religion, Universal Religion, Religion as Cosmic and Human Motive. In his treatment of these themes the author is not so much concerned to show that the one has developed into the other as endeavoring to show that distinct types of religion may be recognized and that these types are well correlated with the respective states of society. Differentiation in social life has been accompanied with a differentiation in religious life, with a separation of religion from the rest of life. "Religion is dependent for its manifestations on the industrial, social, and political developments of a people. As these change the religion changes to meet the new conditions."

We think it safe to say that no book treating of religion has appeared since Durkheim published his "Elementary Forms of the Religious Life" which is so stimulating as the one before us. The author shows a balance and steadiness, a thorough knowledge of most of the great religious systems and of the speculations of writers on religion, which has been but seldom attained. Sanity is the penetrating tone, and every sentence is clear and telling. The chapters are excellently balanced and the argument is well put.

If the book is written 'for a purpose,' that purpose seems to be to

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inspire a new religion. As man has made god in his own image, so he can remake him. The recasting must be in the language of the present age, not in that of the sixth century B.C., nor in that of the Augustan age when belief in prodigies and portents was the language of common thought.

What man has made, man can make again. He has created many a spiritual world in the past, and he can build more stately mansions for the soul in the years to come. The old creations were visionary, largely unreal, of the substance of dreams, shot through with nightmare visions; but the newer realism of the spirit will be finer, with sounder basic foundations in human nature, and with loftier possibilities for the advancing of human interests. Religion, therefore, is not passing away, but coming into its own. It may, and doubtless will, lose the supernatural and the miraculous, its saints and its prophets; but it will gain in the multitude of its faithful men and women, in those who have the qualities of the hero, and who can give themselves unstintedly for the service of their fellows. We may welcome with joy the day of this new and more beauteous religion, for it means that what belongs to human welfare will grow marvellously in every part of the world, and among all the races of men.

This may not furnish much cheer to the orthodox. But those who have read the author through with sympathy will not have much orthodoxy to mix with cheer.

WILSON D. WALLIS

## NORTH AMERICA

*Preliminary Account of the Antiquities of the Region between the Mancos and La Plata Rivers in Southwestern Colorado.* EARL H. MORRIS. (Thirty-third Annual Report of the Bureau of American Ethnology, pages 155–206, plates 31–75, figs. 1–11.) Washington, 1919.

This paper describes excavations carried out by Mr. Morris in the summers of 1913 and 1914 for the University of Colorado. The region investigated is a plateau bounded on the east by the Mancos river and on the west by the La Plata; or, archaeologically speaking, a part of the northern San Juan area lying between the Mesa Verde and Aztec districts.

The first section of the report concerns itself with certain ruins in Johnson cañon, an eastern tributary of the Mancos. The sites are cliff-houses identical, as the author points out, in everything but size with those of the Mesa Verde, so well known from the writings of Nordenskiold and Fewkes. The descriptions are detailed and accurate and the minor antiquities recovered are fully illustrated. There is one mis-

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identification: the wooden object in pl. 47, described as the plunger of a fire-making apparatus is in reality the wooden foreshaft of a reed arrow, as are the two other specimens figured in pl. 44, *g*, *g'*. Dr. Fewkes in his report on Sprucetree House (Bull. 41, B. A. E., fig. 20) has also misidentified a similar foreshaft by calling it a "wooden needle."

The particular interest of the report lies in Part II, "Ruins on the Mesa." The admirably presented data may be very briefly summarized as follows: the sites lie on the crests of ridges; they consist of small, irregularly rectangular rooms placed side by side in single or double rows or in hollow rectangles; the long axes of all the groups run east and west. The bottoms of the rooms were sunk into the ground to a depth of from a few inches to two feet. The hard earth sides of these excavations were sometimes merely plastered, sometimes reinforced by lining them with upright stone slabs; in other cases shallow trenches were dug about the sides of the rooms and double rows of small slabs set into them as wedges to hold upright poles. Although these foundations vary somewhat in detail, the superstructures all seem to have been the same: thin walls of vertical poles, wattled together with osiers and daubed with adobe; the roofs supported by stout posts set at the corners of the rooms. There was never more than a single story. While no kivas appear, there are in the neighborhood of some of the villages depressions which are probably the remains of such circular subterranean rooms as were observed by Mr. Morris 70 miles further east; the latter had central firepits, but none of the other typical attributes of kivas. To the south of the buildings in each case there lay a refuse and burial mound; the bodies were placed flexed in shallow pits, and were accompanied by mortuary offerings of pottery. All the skulls from the graves on the plateau exhibited occipital deformation; those from two apparently similar, though unexcavated, sites (Long Hollow, nos. 22 and 23) in the valley of the La Plata were all undeformed.

The stone and bone artifacts from these "pole-and-mud" villages do not differ greatly from parallel types found in the cliff-houses; the manos and metates (fig. 6), however, seem to the reviewers to be of a less specialized type than those of the cliff-houses. The pottery, on the other hand, is unmistakably different in technique, form and ornamentation from that of the Mesa Verde cliff ruins and the large Aztec pueblo. The ware is coarse, much of it undecorated; the bowls have thinned rims (not square-edged as on the Mesa Verde), and a number of peculiar gourd-shaped vessels appear. The decorations of the painted ware are bold and free (especially on the pieces from Long Hollow) and show little of the conventional geometricism of late Mesa Verde ornamentation.

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In summing up Mr. Morris concludes (p. 204) that the plateau remains are probably to be considered as belonging to the pre-Pueblo culture now generally conceded to have existed in the Southwest. In this we agree with him, for there is no doubt that his plateau sites belong to the same cultural horizon as do the ruins uncovered by us in northeastern Arizona and called "Slab-house" (Kidder-Guernsey, Bull. 65, B.A.E., pp. 41–45). Of the pottery from the two groups, the decorated ware is very similar and the cooking vessels with broad coils at the necks are identical. The houses found by us were oval rather than rectangular; and their superstructures were, partly at least, made of superimposed courses of adobe "turtlebacks" instead of "wattle-and-daub;" the slab foundations, however, were precisely the same. Furthermore, there was associated with one of our ruins a round subterranean room presumably similar to the round rooms that Mr. Morris records from his district.

The above identification of the two groups (Morris' pre-Pueblo and our "Slab-house") as closely related and therefore probably approximately contemporaneous, is interesting as showing that the two cultures were not strictly local ones. It also proves correct Mr. Morris' deduction that his sites are of earlier date than the Mesa Verde cliff-dwellings; for our "Slab-houses" were buried beneath cliff-house rubbish, and cross-finds of traded pottery enable us to state positively that the cliff-houses of our Kayenta region and those of the Mesa Verde were inhabited at the same time.

To return to a point of detail. Mr. Morris states (p. 204) that of 33 crania 11 are undeformed, but he fails to stress the fact that these 11 came from a single group of ruins (Long Hollow, see p. 194); nor does he mention what seem to be rather marked differences between the Long Hollow pottery and that of the plateau sites. The reader of the report cannot judge for himself whether or not these differences are constant, because the exact provenience of the pieces of pottery figured is not given in the plates, and a search of the text serves to locate only 21 of the 51 specimens illustrated. This is a really serious omission, for every scrap of data on the obscure early periods of pueblo history is of value and it is quite possible that Mr. Morris is dealing with more than one type of remains.

We emphasize the above because the designs on such of the Long Hollow vessels as are definitely located bear what seem to us very striking analogies to basketry designs. If this apparent resemblance proves on closer comparison to be a real one, it will be of great interest in throwing

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light on the hitherto mooted question of the ancestry of Southwestern pottery. It was claimed by Cushing and Holmes many years ago that corrugated or coiled pottery, so typical of the Southwest, was the direct descendant of coiled basketry; this view was generally accepted and has often been quoted in works on the development of technology. The fallacy of this idea was pointed out by Mr. Morris (Amer. Anthrop. N.S., vol. 19, pp. 24–29); and the reviewers, working independently, reached the same conclusion (Bull. 65, B.A.E. p. 141). Technologically, then, Southwestern pottery is probably not descended from basketry; but, on the other hand, we now seem to have evidence that on the artistic side the earliest pottery may owe something to basketry. It should be remembered, in this connection, that the "Basket-makers," the oldest Southwestern people of which we so far have knowledge, had apparently no pottery, but made excellent and well decorated baskets; furthermore, they, like the Long Hollow people (whose pottery designs seem to us to suggest basketry prototypes), did not practise skull deformation. Some connection between the two cultures may be suspected.

While the above is, of course, more or less speculative, it is susceptible of proof or refutation by continued field-work on the lines along which Mr. Morris has made so promising a beginning. The origin and development of the Pueblo culture form the one problem in North American archaeology which, because of abundant remains and favorable climate, we have reasonable hope of tracing out clearly in all its technological details. The present paper gives us a wealth of data on one of its most obscure phases. It is to be hoped that Mr. Morris, whose knowledge of the archaeology of the San Juan is unrivaled, may be enabled to continue his most fruitful researches.

A. V. KIDDER, S. J. GUERNSEY

## SOME NEW PUBLICATIONS

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——. El Triangulo facial de los craneos vascos. (Memorias de la Real Sociedad Español de Historia Natural, vol. X, pp. 359–404.) Madrid, 1917. 14 figs., 1 pl.

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——. Craneometría de un Escafocéfalo Guipuzcoano. (Asociacion Española para el Progreso de las Ciencias: Congreso de Sevilla.) Madrid: Eduardo Arias, 1917. Pp. 11–33, 8 figs.

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# DISCUSSION AND CORRESPONDENCE

## COMMENTS: "THE CENTRAL ARAWAKS"

IN a recent issue of the *American Anthropologist* I came across a favorable notice of Professor Farabee's work on the Central Arawaks of British Guiana, *i.e.*, the Wapisianas, the Altarois, and Tarumas. Unfortunately, however, it has been reviewed by a gentleman without either intimate or personal knowledge of the subject, and hence many an error has unfortunately remained unnoticed. It is to remedy these defects and to prevent them remaining on record as authoritative that I feel myself forced to take up my pen, and much against the grain, to write a few lines of destructive criticism.

The temporary shelters made by the Wapisianas are described as having their roof made of the long broad leaves of the troolie palm (p. 18), but this particular tree (*Manicaria saccifera*) does not happen to grow in the hinterland of our colony. So again, the cassava-press is said to be a wicker-woven cylinder made of closely plaited strips from the midrib of the long leaves of the cokerite palm (p. 21). As a matter of fact, the midrib of the *Maximiliana regia* is of such a stiff consistency as to render its application to such a purpose absolutely impossible, its main use being to supply darts for the blow-gun. The press is invariably made from the split stem of itiriti, etc., a species of *Ichnosiphon*. The cassava sieves (p. 22) and pack baskets (p. 22) are similarly never made from the midribs of the palm leaf, but from these split stems. That the Wapisianas ever learned the art of spinning and weaving cotton from the Macusis, etc., (p. 26), is likewise extremely improbable for Mr. Melville, who has spent upwards of thirty years of his life amongst these very same people, has never heard it mentioned by them even as a legend. The leaf of the pine-apple plant (p. 27) is certainly never used for fibre: the author is referring to the Kuruwa, Korowa, etc., the preparation of which fibre has already been thoroughly described by the reviewer, with illustrations in the *Journal of the Royal Anthropological Institute*. *Sipo* (p. 30) is the Brazilian term employed to denote bush rope. A very extraordinary mistake is the statement (p. 42) that the Arapaima (*Sudis gigas*) and Aimara (perhaps Haimara would be better) are fish not found on the Amazon side of the divide. Schomburgk, in the early forties, had already mentioned the great trade done in salted Arapaima



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(the Warapaima of the Macusis) on the Amazon side, and its transport to the market at Para. The author describes a couple of methods of catching smaller animals and deer; in the former case by means of a loop placed in the runway through which the creature's head is supposed to pass, and in the latter by stretching a net across the path along which the deer is forced to run by setting fire to the grass behind (p. 53). Unfortunately neither of these devices have been hitherto seen or heard of in the whole of the area under consideration. Furthermore, on the same page the illustration of the birdtrap lacks the upper portion of the peg supporting the structure, and upon which the whole delicacy of the trap depends.

Again, the form of spring-basket fish trap, excellently illustrated no doubt, is unknown amongst all three tribes: it is met with amongst the Wai-Wais further to the southeast.

The value of the list of fish poisons (pp. 61–64) is inappreciable in view of the absence of any scientific identification of the plants so employed, although several of them have been already described, and it would have been preferable to defer publication of the whole work until those of the fourteen out of the twenty-four despatched for the purpose to the Department of Agriculture at Washington had come to hand. It is true that one or two of the poisons mentioned are traceable, but yet errors seem to have crept in here. *Puraunun* (p. 62) is certainly not a thistle, but an agave; the bulbs, and not the seeds of which are used as mentioned. *Haiarri* is undoubtedly a species of the universally used *Lonchocarpus*, and *Haiarri Kupa* (p. 63) is evidently intended for the *Haiarri* used or found in the river bays or bights, *e.g.*, (*Macusi Kuba*) of the Rupununi, etc., while *Haiarri bali* (p. 63) simply means a plant like or similar to (Arawak *balli*) the *Haiarri* itself.

The bibliography is far from complete, and it is certainly puzzling to know why the name of Roth, who so far has published nothing concerning the three tribes under review, should be included in it.

As to the remaining portions of the text, the present reviewer has the authority of Mr. Melville, to whom, and to Mr. Ogibrie, the Professor rightfully admits his main sources of information, for stating that the legends interpreted by him for the author are now scarcely recognizable, and that the Wapisiana vocabulary is hopelessly inaccurate. How much reliability can therefore be placed upon the grammar and language of the Central Arawaks? The author should remember that the history and language of any of our Guiana tribes is not to be picked up by a few months' cursory travel, with notes and queries obtained enroute, even when the expedition is backed by a lavish expenditure of money.

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He has attempted too much in the comparatively short time at his disposal and the result has been a failure. There is one bright spot however to lighten up the background, and this lies in the excellence of the illustrations. One plate (No. VII) however requires explanation: here it is described as "Wapisiana women weaving a hammock" though the author has published the identical picture in a previous work (*Philadelphia Museum Journal*) where it is represented as showing Maopidian females. Which is correct?

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## ARE THERE EVIDENCES OF AN IROQUOIAN MIGRATION WEST OF LAKE ERIE?

IN the issue of the *American Anthropologist* for September, 1919 Mr. Langford described a village site on the Kankakee river in Illinois and called attention to the fact that its culture differed radically from that of other nearby sites.

Briefly summarized the site consisted of a deep deposit of black refuse earth which contained animal bones in large quantity and numerous artifacts made of stone and clay. Chert arrowpoints were abundant, it being estimated that fifteen hundred had been taken from the site. With a very few exceptions, which Mr. Langford considered intrusions, these points were all tiny, keen triangles without tangs or notches. Potsherds were very abundant. The fragments showed that the entire vessels were round bottomed, with constricted necks and narrow collars. Amongst the decorations on the fragments were notches on the rims, and a chevron arrangement of impressed lines. Amongst the bones of various food animals there were found numerous artifacts made from bone and antler. No European articles were disclosed, nor articles made of polished slate, such as the "butter-fly stones."

Mr. Langford called my attention to the peculiarities of this site after reading my article in the issue of the *American Anthropologist* for December, 1916, "The Characteristics of Iroquoian Village Sites of Western New York." He was struck with the similarity between the characteristics of Iroquoian culture and that of the Kankakee site. In his article he spoke of these resemblances and expressed his opinion that the deposit "represents an early stage of Iroquoian progress eastward." . . .

There is no doubt that he is entirely right in considering this site of Iroquoian origin. In my article I had noted as certain constants of

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Iroquoian culture deep refuse heaps; abundance of articles made of bone and antler; the general use of small triangular arrowpoints; well made round bottomed pots decorated with bands of triangles filled in with parallel lines, making a sort of chevron design; and the absence of large notched points, and of polished slate articles.

Judged from these constants Mr. Langford's site is easily identified as Iroquoian. Its numerous triangular chert arrowpoints, its relatively numerous bone and antler artifacts; its chevron designs and its deep refuse heaps can be attributed only to some Iroquoian people; and there can be little doubt that this site on the Kankakee river far from the abodes of any Iroquoian nation during historic time is the site of a village of some nation of Iroquoian stock.

Although it is simple to establish the identity of this site as Iroquoian, it is far from easy to identify the particular nation which occupied it, or to explain its location so far from territory usually ascribed to the Iroquoian nations. There are two theories to account for the location, both sound, and one of these might identify the nation which occupied it.

During the first years of contact between the French and the most western member of the Iroquoian family, the "Neutral Nation," the French described the wars which at that time were being waged by these "Neuters" on nations of non-Iroquoian stock to the westward, the Mascoutins, and the triumphant return of Neuter war parties in 1638 and in 1643 with long trains of prisoners. It is entirely possible that during one of these wars a large Neuter war party established a camp in the enemy country and there persisted for a period long enough to lay down a deep refuse deposit; and it may be that the Kankakee site is such a camp.

It might be urged that the distance between the villages of the Neuters in Canada and the Kankakee river is too great for such a possibility. In 1651 when they were driven from their country by their fierce kindred, the Five Nations of New York, they were seated in the Canadian peninsula north of Lake Erie between the Grand river and the Niagara, with an advanced band of a few villages east of the Niagara. Nearly all these village sites are of post-European age, therefore long post-dating the site on the Kankakee river which is pre-European. But the Stone Age sites of the Neuters are much farther west, nearly every site of pre-European age being west of the Grand river and extending westward practically to the Detroit. From the Stone Age Neuter sites near St. Thomas and London to the Kankakee river the distance is nearly three hundred miles, a long distance for a war party to operate through hostile

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country. Still, Seneca war parties from their villages two hundred miles still farther east devastated the country of the Illinois, operating through a much greater distance. The distance, therefore, does not preclude the possibility of the Kankakee site being of Neuter origin.

An equally sound theory, and withal a most interesting one, is that the Kankakee site marks a stop in the eastward migration of one of the members of the Iroquoian family. Mr. Langford mentions this possibility. If it be shown to be at least possible, it opens up a very interesting field of research in the country lying between the Kankakee and the Detroit; and every fact known about the migrations of the Iroquoian nations seems to strengthen this theory.

That the Iroquoian nations of Canada, New York, Pennsylvania, and Georgia migrated to these places at some comparatively recent time is well recognized; and that they drove away and dispossessed earlier inhabitants of different stock is equally well known. The migrations of some of these nations have been studied, and have been proved to be from the westward.

Systematic study of the Neuter sites by the writer has shown that these at least migrated eastward from the Detroit river. The Neuter villages west of the Grand river are all pre-European. The first evidences of contact with Europeans, which took place in the decade of 1615–1625, are to be found in the sites east of the Grand river; and all the sites on both sides of the Niagara river show evidences of much European influence. These villages were inhabited in the decade of 1640–1650. In 1651 they were overwhelmed by the Senecas and their allies; were driven back toward the west; and eventually united with broken fragments of the Hurons and others to form the Wyandottes.

Allied to these Neuters and probably an off-shoot of that nation were the Wenroes. These had pushed eastward across the Niagara ahead of the main body of Neuters, and had established themselves in three village groups, the most advanced of which was at Oakfield, only thirty miles west of the Genesee river, and thus almost in touch with the Senecas. In 1639 after a period of constant warfare with the Senecas, the Wenroes abandoned their villages and emigrated to the Huron country for protection from their ferocious kindred.

Amongst the nations speaking a dialect of the Iroquois tongue the Jesuit Fathers listed in 1635 a nation known to them as the Eries. This nation lived south of Lake Erie. Like the Neuters on the north of that lake they seem to have been slowly pushing eastward. Their latest village was at Ripley, New York, in the graves of which a few

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European articles were found. Their earlier villages are much farther west, the nearest identified positively as Erie being at Willoughby, Ohio. This is pre-European. Others seem to be located between Cleveland and Toledo.

The Seneca Nation had entered their New York homes only a short time before the advent of the Europeans. Their movements from their latest Stone Age villages to their historic villages of 1687 are well known. Their earlier movements, which ended with their occupation of the country between Canadaigua lake and the Genesee river, are well marked by one line, possibly two lines, of hill top forts leading from the southward up the valley of the Genesee, thence from the westward over the hills of Cattaraugus and Chautauqua counties to a point south of Dunkirk, New York, where their culture seems to merge with an early Erian type. Certainly this nation then, migrated as did both the Neuters and the Eries, from the westward.

The migrations of the Andastes, the Tuscaroras and the Cherokees have never been studied. There seem to be evidences of similarity between the pottery of Stone Age Andastes and Stone Age Eries; and it is entirely possible that these Andastes were an offshoot of the Eries who entered the valley of the Susquehanna by means of some one of the branches of the Allegheny river.

Neither has there been any study of the early archaeology of the Hurons or the Tionnondadies, yet every evidence points to their immigration into their historic homes from the west. Certainly both were relatively late comers into a country occupied by Algonkian nations. Certainly advanced bands moved eastward during pre-European times and established themselves on the St. Lawrence river, where they were met by Cartier. Certainly in the eighty years intervening between the visits of Cartier and Champlain these bands moved southward into New York where they became known as the Mohawks, Oneidas, and Onondagas. These bands certainly came from the west as far as the St. Lawrence. There is no evidence that the main band of the Hurons came from the north, and their movement northward from New York would have been limited by Lake Ontario. Therefore there is good reason to suppose that they like the Neuters, came from the westward, across the Detroit or St. Clair river, into their historic seats in Canada.

Eliminating however as doubtful the possible movements of the Hurons and the southern confederations of the Cherokees and Tuscaroras, there is still indubitable evidence that there was a migration eastward of strong members of the Iroquoian family of nations, which were to crystal-

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lize into the Neuter, Erie, and Seneca nations. The route of the migration of these nations at least seems to have been eastward from the head of Lake Erie, one band crossing the Detroit river and following the northern shore of Lake Erie, the other following the southern shore; and it is noteworthy that the characteristics of Iroquoian culture are as marked and constant at the most western and earliest of sites along this migration path as at the eastern and later end. From this we can only infer that this culture did not originate on either side of Lake Erie but was already fixed before the Iroquoian people had approached it.

The place of origin of these people is unknown. The movement of these nations however having been uniformly eastward so far as we know, and their peculiarities of culture having been fixed before they arrived at their historic seats in different parts of a wide stretch of country to which their eastward movement had brought them, it is fair to assume that there must have been an earlier movement which brought them to the head of Lake Erie, and that this movement must also have been from the westward. Their place of origin must then be looked for at some point to the westward of the Detroit river.

It is just at this stage of reasoning that the Kankakee site becomes of interest. It is west of the Detroit river, and therefore in a line with a possible eastward movement having that river as its eastern terminus. It is pre-European. It is undeniably of Iroquoian origin. There is then the possibility that it marks a stopping place of a band of some Iroquoian nation in its early eastward migration from its point of origin at some unknown western point.

Should this be true other similar sites may be expected to exist between the Kankakee river and the Detroit river. None has been identified, yet this may be due to the fact that northern Illinois, Indiana, and Ohio have never received any systematic study. An attempt to locate and identify any such possible sites would constitute a very definite piece of research work for archaeologists of that territory. This can be accomplished by an examination of existing local collections of Indian articles. Should this reveal any articles of the typical Iroquoian culture, triangular points, chevron designs on pottery, or an abundance of articles of bone and antler, the place of origin of these articles should be located and thoroughly examined and the results of the examination should be carefully compared with the constants of Iroquoian culture.

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## DID THE SO-CALLED CLIFF DWELLERS OF CENTRAL ARIZONA ALSO BUILD "HOGANS"?

SPREADING west from the desert of the little Colorado river upward toward the pine clad slopes of the San Francisco mountains the evidence of an ancient population is everywhere apparent. Ruins of large pueblos and forts crown some of the cinder cones. Small isolated castles top the crags in the canyons. Cliff houses cower under overhanging ledges while the remains of small stone houses and pueblo-like groups of houses lie scattered over the gray plains among the cedars and the pines.[1]

Potsherds gathered from these sites have the same characteristics, black geometrical designs on a white slip, black geometrical designs on a red slip, corrugated ware and red bowls with polished black interiors. This complex of pottery has associated with it in the canyons, burials containing skulls showing a flattening of the back of the head. This is a characteristic of the cliff-dweller culture as described by Kidder.[2] The pottery except for the red bowls with the black interior is similar to that found by Kidder in the Marsh Pass region. A few potsherds similar to those described by Kidder as belonging to another type which he has called the slab-house culture have been gathered on some of the sites.

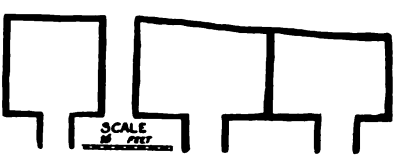



FIG. 14.—Plans of single and double vestibuled houses. The single houses scattered through the pine forests at the foot of the San Francisco mountains, over the sage brush plains and lava flows, and on the crests of the cedar clothed craters are common. The twin houses are rare and have been found in but two of the larger groups or villages. In both types of houses the characteristic feature is the vestibule or entrance on the south or southeast side. The houses figured here lie one quarter of a mile north of the Flagstaff City reservoir.

Among the ruins of pueblos and small houses at the base of the San Francisco mountains, in groups in the pine forest or in the cedar and piñon groves, usually overlooking a flat or a natural park, regular rows of boulders, strike the eye of the careful observer. Similar boulder sites have been described by Mindeleff in the neighboring Verde valley.[3] The rows of small boulders enclose squares ranging from fifteen to twenty-feet on a side. A characteristic of these squares, which has not been

[1] M. R. F. and H. S. Colton, "Little known Small House Ruins in the Coconino Forest," *Memoirs, American Anthropological Association*, vol. v, no. 4, 1918.

[2] Kidder and Guernsey, "Archeological Exploration in Northeastern Arizona," *Bulletin* 65, *Bureau, American Ethnology*, 1919.

[3] C. Mindeleff, "Aboriginal Remains in the Verde Valley, Arizona," *Thirteenth Annual Report, Bureau of American Ethnology*, 1891–1892, 1896, p. 237.

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noted before, is that the row of stones forming the south or southeast side is broken by a gap about six feet wide and from this gap two lines of stones project perpendicular to the side of the square. (See figure 14.) When we consider that these squares are the plans of dwellings of some sort, the parallel walls beside the door suggest the vestibule of a house. Although these ruins are fairly common the writer worked two summers in the region before he recognized them as a distinct type.[1]

In north central Arizona two Indian tribes build earth lodges—the Navajo and the Havasupai. In common with the Navajo hogan our ruin shares the door on the southeast side. The size of the room more nearly approaches that of the hogan than that of the small stone houses that also dot the region. Except for the line of small boulders which mark the sites no trace of the building material exists. The ruins of a Navajo hogan is a circle of soil on the ground surrounding a depression. Our ruin is a square or rectangle of stones surrounding a depression. To this extent the two are similar.

The Havasupai, who by tradition give as their ancient home the San Francisco Mountain region, build hogan-shaped shelters of willow branches in Cataract canyon. On the plateau above, these shelters take the form of earth-lodges. Some of these are round and some square but none were seen where the ground plan would indicate a vestibule.

It might be well to note that the vestibule is regularly a feature of some round earth-lodges such as those of the Pawnee. From the evidence presented above we can infer that our dwelling like the Navajo hogan, was

[1] The largest groups are located as follows:

Two groups north of Flagstaff reservoir on the Schultz Pass road. Township 22 N-R7E Section 33, Twelve houses seven of which show the vestibule.

Babbitt pasture, two houses, Township 21 N-R7E Section 4.

Top of Turkey Tank Mt., one distinct house, five others indistinct. Township 21N-R9E, Section 11.

Dead man's flat, two houses distinct, three others indistinct. Township 24N-R8E, Section 29.

Dead man's flat, three houses, two distinct other indistinct. Township 24N-R8E, Section 28.

Dead man's flat, one house distinct, two others indistinct. Township 24N-R8E, Section 28.

Two miles south of Fortress Mt., two houses distinct. Township 21N-R9E, Section 22.

House in spatter cone SE of Red peak. Township 22N-R9E, Section 33.

Dan Francis estate, two houses. Township 21N-R7E, Section 9.

Elden spring, one house distinct, many indistinct. Township 21N-R7E, Section 11.

South of Dry Lake Mt. one house distinct, others indistinct. Township 22N-R7E, Section 34.



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constructed of logs and earth; and that the body of the house was usually square, like some Havasupai lodges, and that a covered passage led to the interior similar to the entrance to the Pawnee earth-lodge. The evidence seems fairly conclusive that we are dealing with an earth-lodge of some sort.

The Navajo hogan and Havasupai shelters appear so have vestibules but the construction is such that the vestibule does not show on the ground plan. Another difference is worthy of note; the ground plan of our dwelling is usually square, not round. However, two round ones were discovered on Deadman's flat. In three instances our vestibuled houses were joined together by a common wall, but in these cases each room had its own separate door facing to the southeast. Such twin hogans the writer has not observed among the modern Indians of the Arizona plateau. Although the boulder sites about the San Francisco peaks differ from existing earth-lodges yet those differences are not great.

Besides the double or twin character of some of the houses, other variations were noted. Even though the door is almost invariably found on the south or southeast side, yet several cases exist were the door faces the west. In a group of houses, for they are most often found in small groups, the house farthest to the southeast has its door facing the other houses of the group. Abnormalities in shape have been recorded such as the two houses having round ground plans. In general the plans are fairly constant.

Trenches were dug through one house in an attempt to discover the fire-place, but the results proved indeterminate. About the San Francisco peaks the alluvial soil contains a large percentage of charcoal even to a depth of twenty feet telling the tale of ancient forest fires. Although charcoal fragments were obtained everywhere in the excavation no definite conclusion was reached as to the position of the fire-place. It seemed reasonably clear, however, that it was not in the center of the room.

Scattered over the sites of the earth-lodges are potsherds no different than those found in the pueblo ruins,—that peculiar kind of pottery complex characteristic of the cliffdweller culture. One can not help but conclude that the earth-lodges and the pueblos were built by the same people.

This is but a preliminary study. Since a few potsherds, similar to those figured by Kidder and Guernsey (*loc. cit.*) and referred to the slab house culture have been gathered on the sites of the earth-lodges it is

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possible that a further study of the remains may throw light on that little-known culture.

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## COMMENTS ON HANDBOOK OF ABORIGINAL AMERICAN ANTIQUITIES (HOLMES)

IN the interest of future readers of Prof. W. H. Holmes' *Handbook of Aboriginal American Antiquities* which has recently been distributed by the Bureau of American Ethnology, permit me to draw attention to the following surprising statements concerning the archaeology of Mexico and Central America, which challenge comment and criticism.

On page 26 under the sub-title "Mexican Analogies" the following statements occur: "Even more diversified and remarkable are the correspondences existing between the architectural and sculptured remains of Middle America and those of southeastern Asia. In both regions the chief structures are pyramids ascended by four steep stairways of stone bordered by serpent ballustrades. . . . . "

The truth is that the so-called pyramids of Mexico and Central America are in reality pyramidal mounds built up to serve as sub-structures for temples, which, in war-time, were used as strongholds. In order to render the sanctuary as inaccessible as possible and to facilitate its defense access to the summit was gained by a single steep stairway only, situated on one of the four sides, the other three being generally subdivided into several terraces the scaling of which was practically impossible.

The single stairway is a common feature of the chief structures of all of the principal ruined cities of Mexico and Central America, *i.e.*, Cholula, Teotihuacan, Chichen Itza, Uxmal, Palenque, Tikal, etc. In 1895 Prof. Holmes recognized the predominant use of the single stairway in his "Archaeological Studies Among the Ancient Cities of Mexico," which is justly prized by Americanists as a standard work of rare value and charm. In part I, on page 33, he figures eight examples of Maya terraces and pyramids, all with one stairway.

In the text on page 32, however, without citing examples, he states "It is not unusual to find two flights and three; and even four flights are known."

The contrast between this statement and his latest assertion that "The chief structures of Ancient Mexican and Central American cities are pyramids ascended by four steep stairways," seems inexplicable, for

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no new stairways have been discovered on important pyramids since 1895 and all chief examples known, which are cited above, show the single stairway.

It is obvious that an analogy based on a resemblance between southeastern Asiatic pyramids with four stairways and obscure exceptional examples of similar American structures is valueless and misleading and out of place in a handbook.

The same must be said of the second analogy Prof. Holmes points out: "Temple walls in both countries are . . . surmounted by roof crests, cupolas of elaborate and even pagodalike design."

What documentary or archaeological evidence is known which shows that ancient Middle American builders ever constructed cupolas, *i.e.*, "hemispherical or semi-elliptical vaults on the tops of circular buildings?"

Under the sub-title "Whiskered Men in Yucatan" (also on page 26) Prof. Holmes points out a third analogy of an extremely doubtful character, which will remain so until he can produce proof that the so-called "whiskers" are not parts of ceremonial masks or headdresses.

It is indeed a pity that, when real and equally striking analogies between Central America and Cambodia, Burmah, and India do exist, those pointed out in the Handbook should require either substantiation or revision as do also other assertions scattered throughout the pages of what should be a standard reference book.

On page 126 the reader is informed that "the greatest of the American pyramids" (that of Cholula) "is 200 feet in height and 1400 feet square at the base . . . and is now occupied by a Catholic Cathedral." The truth is that the pyramid is surmounted by a church, the only cathedral in the state and diocese of Pueblo being the magnificent one in the city of Puebla, which is 323 feet long and 101 feet wide. According to the careful measurements made by A. F. Bandelier in 1881, quoted by Thomas P. Janvier in his guide, the lines of the base of the pyramid, including their irregular windings, measure are as follows: north line, 1,000 feet; east line, 1,026 feet; south line, 833 feet; west line, 1,000 feet. . . . The length of the upper plateau from east to west is approximately 203 feet; and its length from north to south, 144 feet. There is a steep ascent with a vertical rise of 71½ feet to the first terrace and a second with a vertical rise of 66 feet to the summit, making a total vertical rise of 137½ feet. It would be interesting to know on whose authority Prof. Holmes bases the exaggerated measurements he records. It cannot but also cause regret that at this more advanced stage of archaeological knowledge Prof. Holmes should hark back to the fancied resemblance between

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the trunk of an elephant and the long curved noses of the human faces which decorate the facades and corners of some Maya buildings. Surely the absence of tusks and the broad mouths garnished with teeth which the Maya sculptors placed under the curved nose offset the latter's "insinuating way of suggesting the trunk of an elephant." Prof. Holmes' statement that "The upturned jaw of the mythical serpent is equally reminiscent of the treatment of the cobra jaw in the far East" appears equally misleading as it ignores the marked differences that also exist.

While fascinating, such "suggestions" and "reminiscences" seem out of place in a handbook intended to convey established facts to students. The same must be said of the illustrations represented as that of an "Ancient Wheeled Toy from a Child's Grave, Mexico" although Prof. Holmes admits that it was found by Charnay in a cemetery which also yielded brass bells. In view of the indications that the grave dated from after the conquest and the significant facts that no similar wheeled toys have been found in pre-Columbian graves and that no other evidence has been forthcoming to prove the use of the wheel in ancient America, one wonders why so unmeaning an illustration should have been introduced into the handbook and why the toy should be designated as "ancient."

In a handbook in which the section dealing with Mexican archaeology is so painfully meagre and behindhand one deplores that the space accorded to this toy could not have been allotted, for instance, to a mention of the beautiful and well-preserved little pyramid temple of Tepoztlan in the State of Morelos or some other noteworthy monument or antiquity.

Another misleading illustration occurs on page 353 where figures 214, 215, and 216 represent "primitive methods of drilling" by means of pointed stones, in order to make perforations. Into this group, a drawing representing an ancient Mexican priest lighting the sacred fire by means of a stick (a fire-drill) is introduced and labelled "Primitive Method of Drilling," giving rise to the natural but wrong inference that, as in the other cases, the drill also had a stone point and was being used for the identical purpose of perforating a hard substance.

I regret to have to draw attention to defects in a work which contains so much that is excellent and deserving of praise.

ZELIA NUTTALL

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MEXICO, D. F.

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## Notes on a Navajo War Dance[1]

The following notes were made at a dance which took place at Kayenta, Arizona in June 1914. They are somewhat fragmentary owing to the fact that features of the ceremony were going on in different places at the same time.

The sick man for whose relief the function was given, had, when a young man, looked upon the bodies of two Navajo who had been killed by the Comanche. His trouble arose from the idea that the evil spirits of these enemies possessed him, as it is said to be "bad" to look upon the body of a person slain by one's enemy. The ceremony was directed by a medicine-man who charged a large fee for his services.

The first part of the ceremony took place at a distance of about three miles from the hogan of the sick man, and had to do with the preparation of the "war wand." This wand consisted of the top of a cedar tree from which the bark and all the sprigs except a bunch at the top had been removed. About this stick tied with a deer-skin string, cut from the hide of an animal that had been strangled or smothered, were bunches of white and green sage, several turkey feathers, two eagle feathers, and a package of "medicine," nature unknown. The whole was decorated with long streamers of red cloth. This wand was carried throughout the entire ceremony, which lasted three days, by a young girl, and I saw her still carrying the wand as she rode away.

The part of the ceremony connected with the preparation of the wand that I saw, consisted of dancing by the men. The dance started about dark, continuing through the night and although there were several hundred Indians assembled at the spot, only a few took part in the dance, the greater number looking on or sleeping about the fires.

The dance consisted of singing by two groups of men who lined up facing each other, each line having a leader at the head who chanted in a high falsetto voice a verse which was repeated by his party in concert. The leader of the opposite side followed with a verse which was repeated by his band, and so on. The singing was accompanied by drums beaten by the leaders. The men in each line stood as close together as possible, the lines surging slightly in time to the drums, the dancers at times taking a few shuffling steps. Men were continually dropping out and others taking their places. Now and then I caught a glimpse of the young girl carrying the plumed wand. To all appearances the dance was a contest of endurance between the two lines of men.

[1] Cp. Franciscan Fathers, *An Ethnologic Dictionary of the Navaho Language*, St. Michaels, Arizona, 1910, pp. 366–376; E. C. Parsons, *American Anthropologist*, N.S., vol. XXI, no. 4 (1919).

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At daybreak the dancers rode with a great firing of guns to a large brush house that had been erected near the hogan of the sick man. They were met a little way out by a party of horsemen also firing guns. This appeared to be a sham battle. Soon all the Indians were assembled around the hogan of the sick man; I saw him rushing about, very busy with final preparation. He looked perfectly well but somewhat dishevelled, as his hair was hanging loose. In the hogan of the sick man, two Indians, who were to enact the part of scouts, were being prepared with elaborate ceremonies that consumed nearly three hours. I was not able to gain entrance to the hogan but was told by Professor Cummings who was more fortunate that a good part of the time was spent in painting the bodies of these men, that frequently the attendants who did the painting paused, after using various pigments, to go through the motions



FIG. 15.—Upper left, dance circle; upper right, dance circle open to the east; lower left, painting wife of sick man; lower right, cleaning feathers to be worn by scouts.

of washing their hands in the sunlight that streamed through the smoke-hole.

At the same time that the "scouts" were being prepared, the wife of the sick man was receiving attention in a brush shelter just in front of the hogan. I was able to observe a part of this ceremony, as the shelter

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had only a roof, though the Indians crowded about and made a clear view difficult at times. The wife sat facing the east with a blanket spread over her knees. At each corner of the blanket sat a woman attendant; at her back squatted an old woman who seemed to have charge of the proceedings. This old woman first drew a black line on the face of the wife extending from the ears around the lower jaw, after which black was applied to different parts of the body. Finally, the arms, legs, and practically the whole body of the wife was rubbed with powdered charcoal.[1] Occasionally as the painting proceeded the old woman would hand the wife some object which was immediately concealed by her under the blanket. An old Hopi bowl filled with some liquid of uninviting appearance was passed to the attendants, each taking a sip. It was afterwards handed to several women who seemed to be only spectators, who took a taste.

While these preparations were in progress 12 to 16 Indians, all men except the young girl with the war emblem and another girl about the same age, danced in a circle (fig. 15) about a handfull of ashes and a slender stick with a wisp of hair attached to it. This wisp of hair was from the scalp of a Comanche killed by the Navajo and had been obtained with great difficulty from a distant part of the reservation.

The dancers shuffled around to the singing of a leader, who also beat a drum made of a small pot with a piece of skin stretched across the top. At the end of each verse the dancers paused. In these stops the circle was always left open to the east; it seemed quite important that nothing should cross or obstruct the east. At one time several women started to ride over well out on the flat, but were warned back by a great outcry from both dancers and spectators. Another time horsemen were sent out to turn back some stock that would have crossed nearly two miles out.

Presently a very old man, nearly blind, came from the hogan, and, guided by an Indian who appeared to be the master of ceremonies, took the stick with the wisp of hair from the circle of dancers and carried it some 300 yards east across the flat, pausing once to make a motion like striking the ground or an imaginary enemy. At the end of this journey the old man's guide asked him in a loud voice, what his name was, but the old man being very deaf did not understand, and the question was repeated louder and louder at which the Indians looking on seemed greatly amused. Finally having received a satisfactory answer the guide returned to the hogan leaving the old man. The two scouts now emerged from the hogan wearing kilts or aprons of cloth, bandoliers, and bracelets of yucca leaves, and having feathers tied in their hair. Their bodies were

[1] According to the Franciscan Fathers, it is the patient who is painted black.

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painted black. These men ran rapidly out to where the old man stood, performed some ceremony over the wand which he had placed on the ground, made motions to the sky with the hands raised palms upward, then ran back to the hogan. What happened after this I was unable to note, except that about this time gifts of cloth and other articles were thrown out through the smokehole of the hogan to be caught as they fell by a number of women who waited in line about the door. The remainder of the day was devoted to horse racing and other games.

The event closed with a girls' dance which started about 12 p.m. The belle of the ball was the bearer of the sacred war emblem which she still retained. The dance was performed thus: each girl selected a partner by force who must either dance or pay. Holding her partner by the arm or blanket she turned slowly backward with the man as a pivot, he exerting himself just enough to maintain their relative position and apparently requiring more or less urging for even this slight effort. Both wore all the time a bored expression, the man looking rather sheepish as well. How long this dance continued through the night I do not know, but I saw no change or pause in the three hours I watched it. The most exciting part of the entertainment was furnished by the horses which occasionally stampeded in bunches of a dozen or more and, blinded by the glare of the fires, ran over anything in their way.

There were six or seven hundred Indians present. A few formed a rather uninterested audience. The greater part, however, spent the the night eating or sleeping. From Mrs. John Wetherell I learned that the very old man who was led out on the flat was supposed to discover the enemy and give the alarm. The scouts then go out and locate the enemy, returning to guide a war party who surprise and drive them away, recovering at the same time a young woman who had been captured in a previous raid. The dance about the wand and lock of hair was a part of the rejoicing at the recovery and the success of the war party, though the sequence of these events do not seem quite in order. The young girl who carried the war emblem represented the rescued maiden.

These notes barely sketch the ceremony, a great many features of which I missed altogether. Others perhaps I misinterpreted from lack of knowledge.

I asked Mrs. Wetherell if the sacred war emblem might be obtained in any way but she said it would be considered a great affront to even suggest such a thing to the medicine-man. His wife however, allowed me to look at it and while I was thus engaged managed to steal my jackknife.

S. J. GUERNSEY

PEABODY MUSEUM,
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# ANTHROPOLOGICAL NOTES

WE regret to record the death of one of our honorary members Dr. Friedrich Wilhelm Radloff, Director of the Ethnographie-Anthropological Museum in Petrograd. Radloff was born in Berlin on January 17, 1837. A long sojourn in West Siberia enabled him to make those profound investigations of Turkic culture which established his scientific fame. Among his works may be mentioned the *Volksliteratur der nördlichen türkischen Stämme* (1896–1900), and *Aus Siberien* (1884, 2nd edition 1893). The latter is a sketch-book, but contains valuable accounts of the Altaian and Kirgiz tribes, a general account of West Siberian shamanism and a report of archaeological excavations.

DR. ROBERT MUNRO, the well-known Scottish archaeologist, died on July 18, 1920 in his eighty-fifth year. His reputation rested particularly on investigations of European lake dwellings. An obituary appeared in *Nature* of July 29, page 685.

ON February 18, 1920 the Anthropologische Gesellschaft of Vienna celebrated its fiftieth anniversary. During the past months a series of lectures has been given under its auspices under the caption "Rassen- und Kulturfragen der Menschheit." Among the speakers were Drs. Pöch, Haberlandt, and Oberhummer.

MR. ALVIN H. DEWEY, President of the Morgan Chapter of the New York State Archaeological Association, has been conducting field-work near Rochester Junction, New York. Among other finds of interest, a pottery jar of Algonkian type was unearthed by Mr. Dewey in what seems to have been a Seneca Iroquois grave. The vessel may have been made by some Algonkian captive of the Seneca.

MR. ARTHUR C. PARKER, New York State Archaeologist, has been continuing his exploration of the early historic Seneca stronghold at Boughton hill, near Victor, N. Y. Two pottery jars of the degenerate period of plastic art among the Seneca, which just preceded the abandonment of the manufacture of ceramics by that people, were discovered; several bone combs, some of them of unusual form, were likewise obtained.



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DR. WM. R. BLACKIE, who is conducting an archaeological survey of Westchester county, New York, for the Museum of the American Indian, Heye Foundation, has located a hitherto unknown village site of the Siwanoy in the International Gardens, near Pelham Bay, and has found skeletons and a number of interesting specimens.

MR. ALANSON SKINNER of the Museum of the American Indian, Heye Foundation, spent part of the months of June and July at Keshena, Wisconsin, among the Menomini Indians, gathering data on their material culture.

DR. S. A. BARRETT of the Public Museum of the City of Milwaukee is still engaged in the exploration of the great walled site and mounds at Aztalan, Wisconsin.

PROFESSOR HEINRICH CUNOW, known in this country for his book on the social organization of the Australians and an essay on the Inca state, has been appointed curator (*Director*) of the South American division of the Berlin Museum für Völkerkunde.

PROFESSOR EDUARD SELER, who celebrated his seventieth birthday on Dec. 5, 1919, was elected to honorary membership in the Anthropological Society of Berlin. This organization also elected the following corresponding members: F. Sarauw (Gothenburg); Robert Lehmann-Nitsche (La Plata); Emil Fischer (Bucharest).

DR. OTTO RECHE, known for his researches along the Kaiserin Augusta River, New Guinea, is lecturing on ethnography at the new University of Hamburg.

DR. LESLIE SPIER made a brief trip to the Diegueño under the auspices of the University of California.

PROFESSOR FRANZ BOAS spent several weeks in the Southwest to continue his investigations of the Laguna language.

PROFESSOR GEORG THILENIUS has been elected Rector of the University of Hamburg.

DR. T. T. WATERMAN, who has been connected with the University of Washington at Seattle for some years, has returned to the Department of Anthropology of the University of California.

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DR. LESLIE SPIER has accepted a position in the University of Washington where he will have charge of the courses in anthropology.

MR. ALANSON B. SKINNER, who has been a member of the staff of the Museum of the American Indian for some years, has resigned to accept a position in the Museum at Milwaukee.

MR. N. C. NELSON and Mr. B. T. B. Hyde of the American Museum of Natural History left New York September 16th for a visit to the ruins of Grand Gulch, Utah. This work is in continuation of the Hyde Expedition of twenty years ago. The present expedition is provided for and accompanied by two friends of the Museum.

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# THE METHODS OF ETHNOLOGY

BY FRANZ BOAS

DURING the last ten years the methods of inquiry into the historical development of civilization have undergone remarkable changes. During the second half of the last century evolutionary thought held almost complete sway and investigators like Spencer, Morgan, Tylor, Lubbock, to mention only a few, were under the spell of the idea of a general, uniform evolution of culture in which all parts of mankind participated. The newer development goes back in part to the influence of Ratzel whose geographical training impressed him with the importance of diffusion and migration. The problem of diffusion was taken up in detail particularly in America, but was applied in a much wider sense by Foy and Graebner, and finally seized upon in a still wider application by Elliot Smith and Rivers, so that at the present time, at least among certain groups of investigators in England and also in Germany, ethnological research is based on the concept of migration and dissemination rather than upon that of evolution.

A critical study of these two directions of inquiry shows that each is founded on the application of one fundamental hypothesis. The evolutionary point of view presupposes that the course of historical changes in the cultural life of mankind follows definite laws which are applicable everywhere, and which bring it about that cultural development is, in its main lines, the same among all races and all peoples. This idea is clearly expressed by Tylor in the introductory pages of his classic work "Primitive Culture." As soon as we admit that the hypothesis of a uniform evolution



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has to be proved before it can be accepted, the whole structure loses its foundation. It is true that there are indications of parallelism of development in different parts of the world, and that similar customs are found in the most diverse and widely separated parts of the globe. The occurrence of these similarities which are distributed so irregularly that they cannot readily be explained on the basis of diffusion, is one of the foundations of the evolutionary hypothesis, as it was the foundation of Bastian's psychologizing treatment of cultural phenomena. On the other hand, it may be recognized that the hypothesis implies the thought that our modern Western European civilization represents the highest cultural development towards which all other more primitive cultural types tend, and that, therefore, retrospectively, we construct an orthogenetic development towards our own modern civilization. It is clear that if we admit that there may be different ultimate and coexisting types of civilization, the hypothesis of one single general line of development cannot be maintained.

Opposed to these assumptions is the modern tendency to deny the existence of a general evolutionary scheme which would represent the history of the cultural development the world over. The hypothesis that there are inner causes which bring about similarities of development in remote parts of the globe is rejected and in its place it is assumed that identity of development in two different parts of the globe must always be due to migration and diffusion. On this basis historical contact is demanded for enormously large areas. The theory demands a high degree of stability of cultural traits such as is apparently observed in many primitive tribes, and it is furthermore based on the supposed correlation between a number of diverse and mutually independent cultural traits which reappear in the same combinations in distant parts of the world. In this sense, modern investigation takes up anew Gerland's theory of the persistence of a number of cultural traits which were developed in one center and carried by man in his migrations from continent to continent.

It seems to me that if the hypothetical foundations of these two extreme forms of ethnological research are broadly stated as

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I have tried to do here, it is at once clear that the correctness of the assumptions has not been demonstrated, but that arbitrarily the one or the other has been selected for the purpose of obtaining a consistent picture of cultural development. These methods are essentially forms of classification of the static phenomena of culture according to two distinct principles, and interpretations of these classifications as of historical significance, without, however, any attempt to prove that this interpretation is justifiable. To give an example: It is observed that in most parts of the world there are resemblances between decorative forms that are representative and others that are more or less geometrical. According to the evolutionary point of view, their development is explained in the following manner: the decorative forms are arranged in such order that the most representative forms are placed at the beginning. The other forms are so placed that they show a gradual transition from representative forms to purely conventional geometric forms, and this order is then interpreted as meaning that geometric designs originated from representative designs which gradually degenerated. This method has been pursued, for instance, by Putnam, Stolpe, Balfour, and Haddon, and by Verworn and, in his earlier writings, by von den Steinen. While I do not mean to deny that this development may have occurred, it would be rash to generalize and to claim that in every case the classification which has been made according to a definite principle represents an historical development. The order might as well be reversed and we might begin with a simple geometric element which, by the addition of new traits, might be developed into a representative design, and we might claim that this order represents an historical sequence. Both of these possibilities were considered by Holmes as early as 1885. Neither the one nor the other theory can be established without actual historical proof.

The opposite attitude, namely, origin through diffusion, is exhibited in Heinrich Schurtz's attempt to connect the decorative art of Northwest America with that of Melanesia. The simple fact that in these areas elements occur that may be interpreted as eyes, induced him to assume that both have a common origin, without

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allowing for the possibility that the pattern in the two areas—each of which shows highly distinctive characteristics—may have developed from independent sources. In this attempt Schurtz followed Ratzel who had already tried to establish connections between Melanesia and Northwest America on the basis of other cultural features.

While ethnographical research based on these two fundamental hypotheses seems to characterize the general tendency of European thought, a different method is at present pursued by the majority of American anthropologists. The difference between the two directions of study may perhaps best be summarized by the statement that American scholars are primarily interested in the dynamic phenomena of cultural change, and try to elucidate cultural history by the application of the results of their studies; and that they relegate the solution of the ultimate question of the relative importance of parallelism of cultural development in distant areas, as against worldwide diffusion, and stability of cultural traits over long periods to a future time when the actual conditions of cultural change are better known. The American ethnological methods are analogous to those of European, particularly of Scandinavian, archaeology, and of the researches into the prehistoric period of the eastern Mediterranean area.

It may seem to the distant observer that American students are engaged in a mass of detailed investigations without much bearing upon the solution of the ultimate problems of a philosophic history of human civilization. I think this interpretation of the American attitude would be unjust because the ultimate questions are as near to our hearts as they are to those of other scholars, only we do not hope to be able to solve an intricate historical problem by a formula.

First of all, the whole problem of cultural history appears to us as a historical problem. In order to understand history it is necessary to know not only how things are, but how they have come to be. In the domain of ethnology, where, for most parts of the world, no historical facts are available except those that may be revealed by archaeological study, all evidence of change can be

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inferred only by indirect methods. Their character is represented in the researches of students of comparative philology. The method is based on the comparison of static phenomena combined with the study of their distribution. What can be done by this method is well illustrated by Dr. Lowie's investigations of the military societies of the Plains Indians, or by the modern investigation of American mythology. It is, of course, true that we can never hope to obtain incontrovertible data relating to the chronological sequence of events, but certain general broad outlines can be ascertained with a high degree of probability, even of certainty.

As soon as these methods are applied, primitive society loses the appearance of absolute stability which is conveyed to the student who sees a certain people only at a certain given time. All cultural forms rather appear in a constant state of flux and subject to fundamental modifications.

It is intelligible why in our studies the problem of dissemination should take a prominent position. It is much easier to prove dissemination than to follow up developments due to inner forces, and the data for such a study are obtained with much greater difficulty. They may, however, be observed in every phenomenon of acculturation in which foreign elements are remodeled according to the patterns prevalent in their new environment, and they may be found in the peculiar local developments of widely spread ideas and activities. The reason why the study of inner development has not been taken up energetically, is not due to the fact that from a theoretical point of view it is unimportant, it is rather due to the inherent methodological difficulties. It may perhaps be recognized that in recent years attention is being drawn to this problem, as is manifested by the investigations on the processes of acculturation and of the interdependence of cultural activities which are attracting the attention of many investigators.

The further pursuit of these inquiries emphasizes the importance of a feature which is common to all historic phenomena. While in natural sciences we are accustomed to consider a given number of causes and to study their effects, in historical happenings we are compelled to consider every phenomenon not only as effect but

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also as cause. This is true even in the particular application of the laws of physical nature, as, for instance, in the study of astronomy in which the position of certain heavenly bodies at a given moment may be considered as the effect of gravitation, while, at the same time, their particular arrangement in space determines future changes. This relation appears much more clearly in the history of human civilization. To give an example: a surplus of food supply is liable to bring about an increase of population and an increase of leisure, which gives opportunity for occupations that are not absolutely necessary for the needs of every day life. In turn the increase of population and of leisure, which may be applied to new inventions, give rise to a greater food supply and to a further increase in the amount of leisure, so that a cumulative effect results.

Similar considerations may be made in regard to the important problem of the relation of the individual to society, a problem that has to be considered whenever we study the dynamic conditions of change. The activities of the individual are determined to a great extent by his social environment, but in turn his own activities influence the society in which he lives, and may bring about modifications in its form. Obviously, this problem is one of the most important ones to be taken up in a study of cultural changes. It is also beginning to attract the attention of students who are no longer satisfied with the systematic enumeration of standardized beliefs and customs of a tribe, but who begin to be interested in the question of the way in which the individual reacts to his whole social environment, and to the differences of opinion and of mode of action that occur in primitive society and which are the causes of far-reaching changes.

In short then, the method which we try to develop is based on a study of the dynamic changes in society that may be observed at the present time. We refrain from the attempt to solve the fundamental problem of the general development of civilization until we have been able to unravel the processes that are going on under our eyes.

Certain general conclusions may be drawn from this study even now. First of all, the history of human civilization does not

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appear to us as determined entirely by psychological necessity that leads to a uniform evolution the world over. We rather see that each cultural group has its own unique history, dependent partly upon the peculiar inner development of the social group, and partly upon the foreign influences to which it has been subjected. There have been processes of gradual differentiation as well as processes of leveling down differences between neighboring cultural centers, but it would be quite impossible to understand, on the basis of a single evolutionary scheme, what happened to any particular people. An example of the contrast between the two points of view is clearly indicated by a comparison of the treatment of Zuñi civilization by Frank Hamilton Cushing on the one hand, on the other by modern students, particularly by Elsie Clews Parsons, A. L. Kroeber and Leslie Spier. Cushing believed that it was possible to explain Zuñi culture entirely on the basis of the reaction of the Zuñi mind to its geographical environment, and that the whole of Zuñi culture could be explained as the development which followed necessarily from the position in which the people were placed. Cushing's keen insight into the Indian mind and his thorough knowledge of the most intimate life of the people gave great plausibility to his interpretations. On the other hand, Dr. Parsons' studies prove conclusively the deep influence which Spanish ideas have had upon Zuñi culture, and, together with Professor Kroeber's investigations, give us one of the best examples of acculturation that have come to our notice. The psychological explanation is entirely misleading, notwithstanding its plausibility, and the historical study shows us an entirely different picture, in which the unique combination of ancient traits (which in themselves are undoubtedly complex) and of European influences, have brought about the present condition.

Studies of the dynamics of primitive life also show that an assumption of long continued stability such as is demanded by Elliot Smith is without any foundation in fact. Wherever primitive conditions have been studied in detail, they can be proved to be in a state of flux, and it would seem that there is a close parallelism between the history of language and the history of general cultural development. Periods of stability are followed by periods

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of rapid change. It is exceedingly improbable that any customs of primitive people should be preserved unchanged for thousands of years. Furthermore, the phenomena of acculturation prove that a transfer of customs from one region into another without concomitant changes due to acculturation, is very rare. It is, therefore, very unlikely that ancient Mediterranean customs could be found at the present time practically unchanged in different parts of the globe, as Elliot Smith's theory demands.

While on the whole the unique historical character of cultural growth in each area stands out as a salient element in the history of cultural development, we may recognize at the same time that certain typical parallelisms do occur. We are, however, not so much inclined to look for these similarities in detailed customs but rather in certain dynamic conditions which are due to social or psychological causes that are liable to lead to similar results. The example of the relation between food supply and population to which I referred before may serve as an example. Another type of example is presented in those cases in which a certain problem confronting man may be solved by a limited number of methods only. When we find, for instance, marriage as a universal institution, it may be recognized that marriage is possible only between a number of men and a number of women; a number of men and one woman; a number of women and one man; or one man and one woman. As a matter of fact, all these forms are found the world over and it is, therefore, not surprising that analogous forms should have been adopted quite independently in different parts of the world, and, considering both the general economic conditions of mankind and the character of sexual instinct in the higher animals, it also does not seem surprising that group marriage and polyandrous marriages should be comparatively speaking rare. Similar considerations may also be made in regard to the philosophical views held by mankind. In short, if we look for laws, the laws relate to the effects of physiological, psychological, and social conditions, not to sequences of cultural achievement.

In some cases a regular sequence of these may accompany the development of the psychological or social status. This is illus-

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trated by the sequence of industrial inventions in the Old World and in America, which I consider as independent. A period of food gathering and of the use of stone was followed by the invention of agriculture, of pottery and finally of the use of metals. Obviously, this order is based on the increased amount of time given by mankind to the use of natural products, of tools and utensils, and to the variations that developed with it. Although in this case parallelism seems to exist on the two continents, it would be futile to try to follow out the order in detail. As a matter of fact, it does not apply to other inventions. The domestication of animals, which, in the Old World must have been an early achievement, was very late in the New World, where domesticated animals, except the dog, hardly existed at all at the time of discovery. A slight beginning had been made in Peru with the domestication of the llama, and birds were kept in various parts of the continent.

A similar consideration may be made in regard to the development of rationalism. It seems to be one of the fundamental characteristics of the development of mankind that activities which have developed unconsciously are gradually made the subject of reasoning. We may observe this process everywhere. It appears, perhaps, most clearly in the history of science which has gradually extended the scope of its inquiry over an ever-widening field and which has raised into consciousness human activities that are automatically performed in the life of the individual and of society.

I have not heretofore referred to another aspect of modern ethnology which is connected with the growth of psycho-analysis. Sigmund Freud has attempted to show that primitive thought is in many respects analogous to those forms of individual psychic activity which he has explored by his psycho-analytical methods. In many respects his attempts are similar to the interpretation of mythology by symbolists like Stucken. Rivers has taken hold of Freud's suggestion as well as of the interpretations of Graebner and Elliot Smith, and we find, therefore, in his new writings a peculiar disconnected application of a psychologizing attitude and the application of the theory of ancient transmission.

While I believe some of the ideas underlying Freud's psycho-

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analytic studies may be fruitfully applied to ethnological problems, it does not seem to me that the one-sided exploitation of this method will advance our understanding of the development of human society. It is certainly true that the influence of impressions received during the first few years of life have been entirely underestimated and that the social behavior of man depends to a great extent upon the earliest habits which are established before the time when connected memory begins, and that many so-called racial or hereditary traits are to be considered rather as a result of early exposure to a certain form of social conditions. Most of these habits do not rise into consciousness and are, therefore, broken with difficulty only. Much of the difference in the behavior of adult male and female may go back to this cause. If, however, we try to apply the whole theory of the influence of suppressed desires to the activities of man living under different social forms, I think we extend beyond their legitimate limits the inferences that may be drawn from the observation of normal and abnormal individual psychology. Many other factors are of greater importance. To give an example: The phenomena of language show clearly that conditions quite different from those to which psychoanalysts direct their attention determine the mental behavior of man. The general concepts underlying language are entirely unknown to most people. They do not rise into consciousness until the scientific study of grammar begins. Nevertheless, the categories of language compel us to see the world arranged in certain definite conceptual groups which, on account of our lack of knowledge of linguistic processes, are taken as objective categories and which, therefore, impose themselves upon the form of our thoughts. It is not known what the origin of these categories may be, but it seems quite certain that they have nothing to do with the phenomena which are the subject of psycho-analytic study.

The applicability of the psycho-analytic theory of symbolism is also open to the greatest doubt. We should remember that symbolic interpretation has occupied a prominent position in the philosophy of all times. It is present not only in primitive life, but the history of philosophy and of theology abounds in examples

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of a high development of symbolism, the type of which depends upon the general mental attitude of the philosopher who develops it. The theologians who interpreted the Bible on the basis of religious symbolism were no less certain of the correctness of their views, than the psycho-analysts are of their interpretations of thought and conduct based on sexual symbolism. The results of a symbolic interpretation depend primarily upon the subjective attitude of the investigator who arranges phenomena according to his leading concept. In order to prove the applicability of the symbolism of psycho-analysis, it would be necessary to show that a symbolic interpretation from other entirely different points of view would not be equally plausible, and that explanations that leave out symbolic significance or reduce it to a minimum, would not be adequate.

While, therefore, we may welcome the application of every advance in the method of psychological investigation, we cannot accept as an advance in ethnological method the crude transfer of a novel, one-sided method of psychological investigation of the individual to social phenomena the origin of which can be shown to be historically determined and to be subject to influences that are not at all comparable to those that control the psychology of the individual.

COLUMBIA UNIVERSITY,
NEW YORK CITY

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# RUINS OF THE HISTORIC PERIOD IN THE UPPER SAN JUAN VALLEY, NEW MEXICO

By A. V. KIDDER

IN 1912 the author visited some pueblo ruins in northern New Mexico which seemed to be of historic date. A paper on them was read before Sec. D of the General Meeting of the Archaeological Institute at Christmas of that year, a brief summary of which appeared in the *American Journal of Archaeology*, vol. XVII, pp. 89–90. Fuller publication was postponed because of the loss of the field notes and because it was hoped that another visit might be made to the sites. As no such opportunity has since arrived, it is thought best to present what data are at hand without further delay.



FIG. 16.—Ruin I, from the Cañon.

The ruins lie in Gobernador and Largo cañons, tributaries of the San Juan which enter it on the south side near the Colorado-New Mexico line. Three groups were visited: two about ten miles up Gobernador cañon on the west rim; and one in Largo cañon about twenty miles to the southwest. Information as to other similar ruins in the vicinity was given by Mexicans, but the short time available did not premit of their investigation.

The three settlements are practically identical in situation and general appearance. They occupy easily defensible positions, being perched on projecting spurs of the mesas, with wide outlooks up and down the cañons and back across the level tablelands behind them (figs. 16, 17). They are secured on the cañon sides by walls



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and by the steepness of the cliffs, while from the mesas they are protected by high defensive walls without openings. None of the groups appear to have contained more than twenty ground-floor rooms. In addition to the normal pueblo-type rooms, there are within the defense walls of each settlement a number of ruined log structures closely resembling modern Navajo hogans (fig. 17).[1] The masonry of the stone-built rooms and of the defense walls is poor, the construction being of irregular blocks of sandstone, seldom carefully shaped and only vaguely coursed (fig. 18). The interiors of rooms, however, where protected by roofs still intact, are neatly and smoothly coated with white plaster. Door-ways are relatively large. In one house there was noted a fireplace of the "hood" type,[2] a feature not found, as far as I know, in any prehistoric ruin. Many of the roofs of the rooms are still in place and show very clearly the use of metal axes. The cut surfaces are such as could not possibly have been made with any kind of stone implement; there were also found a number of heavy hewn planks of a type quite unknown in pre-Columbian buildings of the Southwest. The amount of wood used throughout these dwellings is indeed remarkable and is doubtless due to the possession of efficient cutting tools. In prehistoric buildings most roofs are made of slim poles, reeds, or bark, supported by two or three heavy beams; in these rooms, on the other hand, the roofs are of good-sized logs or hewn planks laid side by side (fig. 18), a process which would have involved an enormous amount of labor if stone axes had been employed for felling and trimming. Further evidence that these buildings are

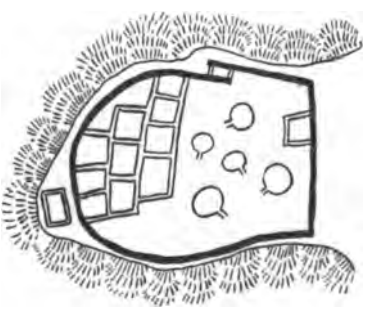


FIG. 17.—Sketch-plan of Ruin II, Gobernador Cañon.

[1] This plan is drawn from memory, the field notes having been lost; it is only meant to show the relation to each other of the houses, hogans, and defense wall.

[2] See Victor Mindeleff, "A Study of Pueblo Architecture: Tusayan and Cibola," *Eighth Annual Report, Bureau of American Ethnology*, p. 169 and fig. 66. The Gobernador example is identical with the left-hand specimen there figured.

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FIGS. 18.—(1) Masonry in Ruin II. (2) Wooden roofs in Ruin II. (3) Hogan-like structure, Ruin II.

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of historic date was afforded by the finding of cow and sheep bones in the rubbish.

The hogan-like structures are built in every case within the enclosing defensive walls, close to the stone houses. They are much decayed, but all show the same general features, being circular in ground plan, eight to ten feet in diameter, and made of cedar logs set in the ground and meeting at the top tipi-style (fig. 18). They seem to have been covered with sod or earth, and to have been entered by short, low passageways of stone roofed with split cedar. From the condition of their beams they appear to be contemporaneous with the stone houses.

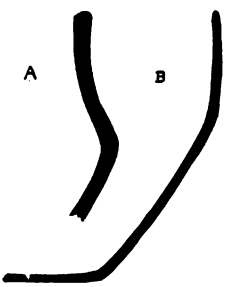


FIG. 19. — Rim-shapes, painted ware bowls.

The pottery of all three ruins is alike. Three wares are represented in the sherd collection:

1. Blackware.
2. Thick two and three-color painted ware.
3. Thin three-color painted ware.

### 1. BLACKWARE

The fragments are all of large ollas with widely flaring, unthickened rims. The exterior surfaces of most sherds have been textured by scraping while still soft with a rough object (corncob?) which has left series of fine parallel striations. The ware is thin; average thickness one-eighth inches. No sherd shows any trace of corrugation.

### 2. THICK TWO AND THREE-COLOR PAINTED WARE

This pottery is not distinguishable, in the sherds at least, from the "modern painted" ware of the Pecos and Tano countries in central New Mexico.[1] The vessels represented are all bowls, most of which have high, recurved rims (fig. 19, *a*). All surfaces are nicely smoothed. Decorated portions have thick yellowish and

[1] See Nelson, *American Anthropologist*, N.S., vol. XVIII, p. 176; and Kidder, *ibid.*, vol. XIX, p. 330.

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grayish slips which tend to "crackle," and are also soft and easily worn away. Undecorated surfaces are well, sometimes highly, polished red. Ornamentation is in dull black (fig. 20, *g*) with occasional red elements (fig. 20, *b*). Fragments average one-fourth inch in thickness.

### 3. Thin three-color Painted Ware

This class is quite different from the former one. The shapes are: ollas (form not ascertainable); and bowls. Of the latter there are two varieties: (*a*) Similar to, though apparently smaller than, the bowls of the thick painted ware, rims high and recurved (fig. 19, *a*); (*b*) Small, deep bowls with rather flat bottoms (fig. 19, *b*). The pottery itself is extraordinarily hard and was evidently very highly fired; most pieces are dark gray in cross-section. The surfaces are not well finished, appearing to have been merely wiped with a cloth or scraped with a piece of gourd rind, rather than to have been worked over with a polishing stone. The color of the bowl walls (whether or not it is a slip is doubtful) is warm yellow to orange. The lower sides and bottoms, both within and without, are carelessly smeared over with a thin red wash, through which the yellow base-

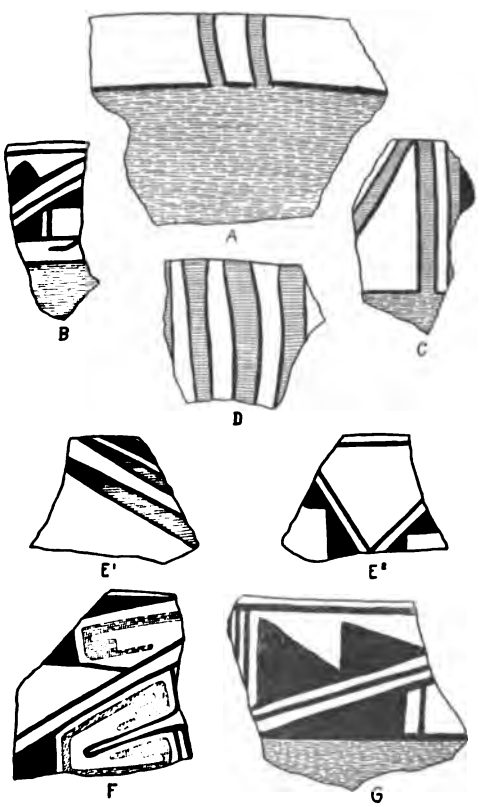



Fig. 20.—Decoration of painted ware.

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color often appears. The upper sides, interior and exterior, bear simple line decorations in red, margined with faint brownish black (fig. 20, *a* to *e*); a few designs are in black alone (fig. 20*e*[2]). The rim-edge is usually painted red. This ware has not yet been found at Pecos; I do not know what its affinities are.[1]

## Conclusions

Two points are obvious from the foregoing; *first* that these houses were built during the Historic period; and *second* that their builders were probably in contact with the Navajo or some other people who made circular, earth-covered lodges of wood.

The comparatively advanced decay of the ruins, and the fact that stone implements were still in use,[2] argues a considerable age.

Two explanations of their origin present themselves: first, that their inhabitants were, so to speak, indigenous, and that iron tools, livestock, etc., were transmitted to them by tribes farther south who were in actual contact with the Spaniards; second, that their builders were members of one of the Pueblo tribes, who for some reason came north, lived in the Gobernador region for a time, and then either returned to their former houses, or were destroyed.

I think that the first theory: namely that the builders of these ruins were indigenous, is extremely improbable from the fact that in the exhaustive lists of towns given by the early Spanish chroniclers, there is no mention of any such northern settlement. A trade sufficiently brisk to have carried cattle nearly to the Colorado border could scarcely have passed unnoticed. All the evidence seems to point to a temporary occupation by some of the known Pueblos.

The date of this occupation and its cause are perhaps to be found in the following historical information summarized from Bandelier's Final Report (part II, pp. 215–216):

> The Spanish, having been expelled from New Mexico in 1680 by a general uprising of the Pueblos; returned in force ten years later, and reconquered the

[1] As it is quite impossible to give a really understandable verbal description of pottery types, the author will be glad to send a selection of sherds to anyone who wishes to make comparative studies.

[2] A number of arrow-points, scrapers, and drills were found in the débris.



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country. At this time they found the Jemez tribe very difficult to control. Again and again these people conspired with other Pueblos and particularly with the Navajos, to harass the Spaniards and the friendly Pueblos of Sia, Santa Ana and San Felipe; and time after time they were punished, their villages destroyed and themselves driven to the high mesas. Finally in 1696 occurred the last important insurrection. A battle was fought in which the Jemez were completely routed, their Pueblo allies from Acoma and Zuñi deserted them, and they fled north to the Navajo country. In the following summer no trace of them could be found in the Jemez valley. They remained away for some time, apparently about ten years, but eventually returned to their deserted towns.

I can find no other account of any Pueblo people having moved so far to the north during or after the revolt; Gobernador cañon is in the old Navajo country and, being nearly a hundred miles from the nearest Spanish settlement of that time, and in a wild and inaccessible district, would have offered an ideal refuge.

The presence of the hogan-like structures in connection with the ruins should also, I think, be taken into account. No such dwellings were ever made, as far as I can discover, by any past or present Pueblos. They seem surely to point to contact with the Navajo, which contact seems most appropriate to the case of the fleeing Jemez.

As to the desertion of the ruins, we must remember that the Navajo were always at bottom the natural enemies of the Pueblos. That the Jemez eventually returned to their none too fertile old range, in close proximity to the hated Spanish, is evidence enough that they were not in the most enviable of positions in the country of their notoriously fickle allies.

A comparison of the pottery found at the Gobernador sites with that from the villages from which the Jemez were driven at the time of the revolt, should definitely settle the question.

If it can be shown that these northern ruins were occupied by the Jemez, we shall have gained, not only an interesting bit of historical information, but we shall also be supplied with most valuable data for the study of the development of Pueblo pottery, for in the case of most historic ruins we know the date of abandonment, but it is usually impossible to say when they were built. Finds in them, therefore, cannot be later than a given date, but, on

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the other hand, they may be very considerably earlier; there is always doubt. Here, however, if further examination confirms the Jemez theory, we will have a segment of culture definitely dated at both ends. Excavation would almost surely yield a sufficient amount of material to give a sound knowledge of the technique and ornamentation of the pottery. This would be of the greatest value, and would form a solid foundation upon which to work, not only forward to the later Jemez valley wares, but also back toward the less clearly defined types made in early historic and late prehistoric times.

PHILLIPS ACADEMY,
ANDOVER, MASS.

NOTE: In 1916 Mr. N. C. Nelson, of the American Museum, incidentally traversed parts of the region in which ruins of the type and period considered in the above paper occur. He observed four ruins on the Largo cañon, one near the head of the Gobernador and three directly east, near the junction of the Burns and La Jara cañons. Four additional ruins were reported to him in the Largo, two or more about halfway up the Gobernador (some of which have been excavated by Mr. Earl H. Morris) and several more were said by the forest rangers to exist on the Carriso and Burns cañons. Traces of ruins, apparently of the same type, were also found to the south, in the Puerco drainage; one being at Cuba and another about two miles west of Casa Salazar. Finally, current reports had it that other "Torreones" exist on the Los Torreones creek to the northwest of Cabezon, but these may as easily be remains of Navajo hogans.

The ruins observed all occupied conspicuous situations on the rimrock, on isolated crags or on detached blocks of rock. Those examined were uniformly small, ranging from two to six rooms. The masonry was of an ordinary type with a peculiar tendency to round off the corners. The timbers, including the notched ladders, had for the most part been cut with a metal ax. One fireplace hood was photographed. Pottery was scarce but the fragments picked up include three-color glazed ware of the Rio Grande type, early historic glazed ware, and a black-on-yellow or pink ware much resembling that found in the Keresan region near Cochiti on the Rio Grande.

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# ESCHATOLOGY OF THE QUILEUTE INDIANS[1]

By LEO J. FRACHTENBERG

## Introduction

OF the numerous phases of primitive life which confront and attract the attention of the student of primitive races, none is more interesting and none yields a better insight into the philosophical concepts of a given set of people, than the attempt, on the part of these people, to explain the mysterious causes which surround a person's death and the speculations concerning the complexion of the next world, and the forms of life assumed in the hereafter by the soul of a departed person. It is the study of eschatology, the investigations conducted into the beliefs, held by primitive races, concerning after-life and the composition of the human being, which reveal to us the deepest and minutest philosophical thoughts of primitive man. And, if it be true that the aptitude for mental achievenent of a given race may be measured by the depth of its speculations into the philosophy of life, its origin and future, as evinced in the logical beliefs held regarding souls and forms of after-life, then the American Indian, and particularly the Indian of the Northwest coast, must be regarded, on the basis of such a study (even if all other criteria were lacking) as having achieved a high stage of intellectual development. It may be safely supposed that a high mental aptitude goes hand in hand with a corresponding aptitude for attaining to a high development of certain phases of material culture, such as pottery, basketry, woodcarving, or certain accomplishments pertaining to a sea-faring life. In this respect, a close parallel suggests itself between Ancient Greece and the various units that go to make up the Northwest Coast area. In both regions, we find a high mentality closely followed by, or to put it differently, effecting, an unusual development of material culture. The Tlingit, Haida, Kwakiutl, Nootka,

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Quileute, and other Indians of the Northwest coast, display certain forms of intellectual life coupled with high cultural achievements which are certainly lacking among the tribes living farther south.

This contrast is particularly strong when one compares the lewd mythology, the woeful attempts at philosophy, and the complete absence of any highly developed phases of material culture among the Indians of Oregon (such as the Coos, Siuslaw, Alsea, Molala, and Kalapuya) with the beautiful tales, the high philosophical concepts, and the accomplished forms of basketry and wood-carving of the Indians north of the Columbia river. In this connection, it may be noted that the farther north from the Columbia river we proceed, the more highly developed (intellectually and culturally) tribes we find. In other words, the Columbia river would seem to form a dividing line between two well-defined and sharply contrasted cultural areas. And, just as in Ancient Greece, or in Italy during the Renaissance, or in Germany during the classical period of Schiller and Goethe, where intellectual and cultural attainments as national assets did not interfere in the least with professions of allegiance to regional governments, so, also on the Northwest coast, the high mental and cultural development was equally shared in by all the tribes constituting that region without affecting, however, their separation into several groups and tribes. In Greece, as well as in Italy or in Germany, we deal with a more or less homogenous people bound together by ties of kinship and language, whose high traits were more national than regional in character. And, may we not assume the probability of eventual proof, that the adaptability of all Northwest Indians for a high type of culture ultimately goes back to a common source which has little, if anything, to do with contact or the influences of environment?

Before proceeding with a discussion of Quileute eschatology, it may be well to say a few words concerning the distribution and history of these people. The Quileute Indians, it will be remembered, belong, with the now extinct Chimakum tribe, to the so-called Chimakuan linguistic family. Earlier writers, and particularly Farrand, assigned three distinct dialects to this group; the Chimakum, the Quileute, and the Hoh. The latter, however,

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according to a detailed investigation carried out by the present writer, shows no particular points of differentiation from the Quileute variety, and the word Hoh may be safely assumed to be a purely geographical term. On the other hand, the differences between Chimakum and Quileute are purely lexicographic in character, as is shown clearly by a comparison between the Chimakum data collected by Boas and Gibbs and my own Quileute material. Slight phonetic divergencies have been found to exist between these two dialects, the most important being the entire absence, in Quileute, of the nasals *m* and *n*, resulting in a regular substitution of *b* and *d* for the Chimakum nasals. Boas has long ago called attention to the close structural, and to some morphological, correspondence existing between the Wakashan (Kawkiutl-Nootka), Salishan, and Chimakuan languages. It will be remembered that his discoveries were based upon very meager data. Since then, extensive data have been collected and digested by Boas and Sapir in the field of Wakashan linguistics; by Boas, Teit, Haeberlin, and others in the field of Salish philology; and by the present writer in the field of Quileute and Makah linguistics. While much of this material is not yet available for comparatory purposes, enough has been published to demonstrate the soundness of Boas' original theory. The present writer has gathered sufficient data upon which to base the assumption, that these three groups of languages are genetically related and that they ultimately go back to a single common source. Tentatively, he proposes to call these linguistic stocks, the Mosan group of languages, from the fact that the numeral four, *mōs* or *bōs*, occurs in some form or other in one or more dialects composing each of the stocks treated hitherto as separate units.

The Chimakum Indians who, as has been said before, are totally extinct today, occupied a small portion of the northeastern part of Jefferson county in the state of Washington. Their Quileute cognates lived until 1854 on a small prairie in the central part of Clallam county in the same state. Since then, they have been moved farther west and occupy today a small strip of land around the mouth of the Quileute river, known as the Quileute Indian reservation. It is situated about 46 miles south of Cape Flattery

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and about 50 miles north of the Quinault reservation. The Hoh Indians (about 30 in number) live some 20 miles south of the Quileute reservation. These Indians are of particular interest to the student of ethnology, because, with their Makah (Nootka) neighbors to the north, they are the only North American Indians known to have actually engaged in whale-hunting, a profession in which they have attained a great skill and high perfection. Whale-hunting was given up by the Quileute Indians some 20 years ago, but most of them are still engaged in sealing which yields a not inconsiderable part of their annual income. As a littoral people the majority of the Quileute are fishermen today, deriving most of their income from this pursuit. However, I hope to demonstrate in another paper,[1] in the course of publication in the *American Anthropologist*, that originally these Indians were hunters, *par excellence*, and that they lived much farther inland than has been the case during the last seventy years.

The material upon which this paper is based, forms part of an extensive study of Quileute ethnology and linguistics, undertaken under the auspices of the Bureau of American Ethnology, and conducted during the summer of 1915 and during the summer and fall of 1916. The writer spent most of this time on the Quileute reservation, located at Lapush, Washington. While collecting these data, particular care was taken to interrogate only such individuals as were known to be authorities in their particular subjects. Thus, the chapter on eschatology was worked out with the aid given, willingly or grudgingly, by the last surviving medicine-men of the Quileute Indians. Two of these proved rather willing informants; a third had to be coaxed and cajoled into giving information; while the fourth refused most persistently and obstinately to "reveal any secrets imparted to him by his guardian-spirit." It was suggested to me that the persistent silence of this fourth potential informant may have been due to utter ignorance on his part; but I have good reasons to believe that his reticence was the result of a misguided conservatism and of actual fear of the possible consequences for revealing sacred mysteries. It is needless for me

[1] "The Ceremonial Societies of the Quileute Indians."

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to accentuate the fact that, wherever possible, the information imparted by one informant was carefully checked up with the aid of the other individuals, and that the data, thus obtained, present a fairly accurate account of the beliefs held by the Quileute Indians regarding the soul, after-life, and the country of the souls.

## The Soul

The Quileutes believe that each human being, animal, and inanimate object possesses a plurality of souls which, upon the termination of the visible existence of their owners, go to the Country of the Souls. These souls or shadows, called *tē'tipa'd*, look exactly like the living being and may be taken off or put on in exactly the same manner as a snake sheds its skin. Generally, the souls complete their journey to the next world without any outside assistance; sometimes, however, the soul of a deceased relative will come up from the underworld to meet and aid the departing soul of a dying member of the same family.

The human being, according to the best authorities, consists of the body (bones and skin) called *libē'kl!is*, whose ultimate fate is of no concern to the Quileute Indians; of an inner soul, called *libitē'-tipa'd* "main, strong soul"; of an outside soul, termed *t!a'χkl!is tē'tipa'd* "outside shadow"; of life, designated *suwā'tcaqo'lowa* "the being whereby one lives"; and of the ghost for which two distinct terms are used. The ghost of a living person is called *t!ōtsa'aqo'lowa* "the thing whereby one grows"; while that of a dead human being is alluded to as *yalā'* "ghost, devil." According to one of my informants, a similar distinction in terminology is made between the outside soul which is still part of a person and between the selfsame soul after its dissociation from the rest of the body, the latter being called *alā'lilā't.sqal*, while to the former was applied the term used above. Inasmuch, however, as the other informants claimed to be totally ignorant of such a distinction and also in view of the fact that the new term is a verbal and not nominal form, I am inclined to look upon this novel differentiation as individualistic and not original.

The combined efforts of my informants failed to bring out

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clearly the exact function of the life-unit and its relative position to the other component elements of a human being. The most they ventured to say regarding it was that,

> It is distinct from the body, looks just like any of the two souls, dwells between the ghost and the outer soul, leaves the body before the departure of the latter, and may be recovered and restored to its owner by a properly qualified shaman.

Far clearer and more definite was the information obtained regarding the other parts of a human being, which may be given as follows:

The outside shadow leaves a person, as soon as he becomes sick, the inner soul departs a day or two before his death; and the ghost leaves the body at the very moment when death sets in. Death can occur only after the departure of either the inner soul or of the ghost; the loss of the outer soul does not necessarily involve death. In other words, the Quileute Indian regards sickness as a result of the departure of a person's outside soul; while death is caused by the loss of the inner soul or of the ghost.

Upon leaving the body, the outer soul goes straight to the underworld. This, however, is not the case with the inner shadow which, before going to the underworld, visits the places frequented by its owner prior to his death and bids them farewell. These wanderings last usually a week and sometimes two. Upon its arrival at the underworld the inner soul is met by the outside shadow, and the two become unified just as they were prior to their temporary dissociation. Only the outside soul can be brought back from the Land of the Shadows and restored to its owner. But if a shaman succeeds in catching the inner soul of a person, while it is still traveling and before it has descended to the Land of the Shadows, he can restore it to its owner who, thereupon, regains his health. Only such shamans are capable of intercepting the two souls who have special guardian-spirits, called *titēpa'dasił*. Such guardian-spirits are usually human beings. As has been said before, the two souls look exactly like the human being who owns them; the outside shadow, however, is somewhat darker than the inner soul. The souls of a person are his individual property and may not be sold.

The ghost of a person is a trifle longer than the rest of the body,

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extending somewhat beyond the toes and above the head. The ghost leaves the body simultaneously with the setting in of death and goes directly to the underworld where it joins the two souls. It will be remembered that, as long as the inner soul is still traveling, its owner is not yet dead. Consequently, his ghost stays with him until the inner soul, having completed its wanderings, arrives in the underworld. At that self-same moment death sets in, and the ghost leaves the body in order to join the two souls. No shaman has the power to bring back a departed ghost, but their guardian-spirits enable them to see and to drive away ghosts. The ghosts sometimes come up from the underworld causing sickness among the living relatives so that these may die and join them. Occasionally they merely visit their former habitations. On all of these trips they are usually accompanied by the two souls. Ghosts travel at night only and may be heard whistling, which is their form of singing. For that reason the Quileute are forbidden to whistle at night, because it is feared this might attract a ghost to a whistling person. Ghosts never like to come up close to the village, for the smell of living beings is repugnant to them. Only shamans have the power to drive the ghosts back into the underworld. This is accomplished by means of exorcisms revealed to the shaman by his guardian-spirit. At night when the common people begin to feel creepy, in the belief that ghosts may be coming, they shoot off their guns and make other noises, in the conviction that this will keep the ghosts away from their households. Should a common person meet a ghost face to face, that person would go into convulsions and die on the spot. Ghosts of dead warriors sometimes act as protectors of the living relatives of these warriors.

The ghost has the form of a human being and is provided with hands, feet, eyes, nose, etc. His body is covered all over with moss, including the face, mouth, hair, and hands. His nose is long and hangs down as far as the chin; his eyes are large, round, and of a yellow color; he walks crooked, crossing and recrossing his legs at each step and can run very fast. He eats and drinks just like a human being. One of my informants claims to have seen a ghost during a trip to the underworld. He could not come close to him,

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however, for the ghost disappeared as soon as he became aware of the intruder.

The Quileute are aware of the fact that the shadow of a person is caused by the sun. A majority of my informants claimed that it has no connection whatsoever with any of the souls; one, however, ventured the suggestion that it may in some way be related to the outer soul.

The general theory regarding death is that it was originated by Raven, and the story told of its origin differs very little from similar tales obtained among the other Indian tribes of this region. The Quileute make no distinction between natural and unnatural causes of death; by this I mean between death caused, for example, by the infliction of a wound and between death caused by the occult powers of some malicious shaman. A person simply dies, because his soul has left him, the causes which brought about this departure being immaterial.

## The Country of the Souls

Having discussed the Quileute beliefs regarding the composition of a human being, we shall now proceed to a description of the Country of the Souls and of the form of life pursued by these souls after their dissociation from the body of their owners.

All souls, whether they belong to male or female beings, to good or bad people, go to the same place and traverse the same trail; excepting the souls of infants. These have a country of their own, which will be described later on.

The dwelling place of the souls is called the Country of the Ghosts and is situated way under the ground. The place and the trail leading to it have often been described by shamans who, accompanied by their guardian-spirits, used to go down there in order to bring back the souls of some of their patients. The journey could be made only by such shamans who had special guardian-spirits for that purpose. These guardians were usually dwarfs between one and two feet tall who, upon the death of one master, came back from the underworld and chose a different owner imparting to him the powers possessed by their previous master.

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The underworld or Country of the Souls is situated very far from the surface of the earth. A shaman traveling there with the aid of his guardian-spirit requires two days and two nights for that trip and, be it remembered, these guardians travel with lightning rapidity. The road is good and broad, and the underworld itself is a large valley with neither hills nor mountains. Through the center of the underworld runs a river, about a quarter of a mile wide, and the souls dwell on both banks of this river, occupying houses exactly like those of the living Quileute. The river divides the underworld into two equal parts. On one side live those souls who have died long ago, while on the nearer bank dwell the souls of recently departed Indians. The river is crossed by means of a canoe, and for that reason the Quileute Indians bury their dead in canoes. The soul of a poor Indian whose relatives cannot afford a canoe-burial, crosses the river by walking on the fishtrap owned in common by all the inhabitants of the underworld. The trail is at first dark and dim but, as one progresses farther down, it becomes lighter and lighter until the Country of the Souls is reached, where the sun shines with the same brightness as in the world above.

At a distance situated about one third from the upper world there stands a house called *t!apeli'ttī* "mat-house" in which the traveling souls stay over night, resting and acquiring new strength for a continuation of their journey. The trail leads right through this house. In the morning the souls resume their journey and reach pretty soon a lake called *L!ō'lē'sida'* "sticky water" which is so situated that each soul must wade through it in order to continue the trip. The water of this lake, when partaken of by a soul, causes the actual death of its owner. Hence, a shaman going in quest of a lost soul, will always refrain from tasting this water. Beyond this lake there are berry-grounds where the souls pick salmon-berries and strawberries. These berries, too, may be eaten by the souls only. A shaman, who even touches them, drops dead. Farther down the road there is a pole stuck into the ground and continually springing over the trail. The souls have no trouble in passing by this pole. But the shaman, in order to pass by it safely, must have a special magic called *yālā'a'lo' taxē'lit* "ghost guardian,"

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which causes the pole to stop its swinging motion while the shaman goes by. A shaman lacking in such a guardian-spirit can go no further and must turn back. Beyond this pole there is another obstruction in the form of a rotten log lying clear across the road in such a way that each person must step over it. And since this log keeps on shrinking and expanding just like a rubber, only a soul or a shaman having the "ghost magic" can go over it. From here on the trail becomes fine and unobstructed, ending at the very river. Extending clear across the river there is a fishtrap in which the souls catch all their fish.

The souls of recently departed people cannot cross at once to the other side. If they do so, they are driven back. They must stay on the nearer side until they have lost all scent of "recent death." The "older" souls have a sentinel on each side of the river called *tcī'alī'q!wayō'* "guardian" whose duty it is to see to it that no "new" soul comes across until the proper time has arrived. As soon as the "new" soul has completed it apprenticeship and becomes "ripe" for dwelling in the "older community," it is instructed by one of these sentinels how to act and what to do in the real Country of the Souls. New arrivals are usually met and welcomed by the souls of those relatives and friends who had preceded them into the underworld.

The grouping of the souls in the underworld corresponds to their grouping in the upper world; that is to say, souls belonging to one and the same family constitute an individual house-group and own and live in a house of their own. The Country of the Souls differs in nothing from the upper world. It is abundantly supplied with all necessities of life. Each soul pursues the same occupation as in the world above. Sickness prevails among the souls to the same degree as among the living Indians, and the soul-shamans are kept constantly busy. Good and bad weather, day and night, changes of the season prevail also in the underworld. Women bear children in the usual way, and wars are not infrequently waged among the souls. The souls have an ocean of their own, thereby giving them the opportunity to follow the whale-hunting profession. All souls stay in the underworld forever.

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Animals, birds, fish, etc., dwell in special underworlds of their own. These are visited by the dead hunters and fishermen who thus obtain their necessary supplies of fresh game and fish. No one knows the exact locations of these places, nor has any living person ever been able to find out how to reach them.

Attention has been called before to the fact that, according to Quileute belief, infants and children live in a separate underworld. This is called *tcitco'otskl'ātal*, is situated south of the Country of grown-up Souls, and has a trail of its own. The same is covered with nice, green grass and is much shorter than the other road. It is not dotted with obstructions and leads right into the habitations of the souls. The whole place consists of a grass-covered valley surrounded by small hills. Swings made of poles are found everywhere and are used by the children constantly. The houses are located right behind the playgrounds. A beautiful lake is situated in the middle of this underworld, and the children bathe in it frequently. It is not known whether and what the souls of infants eat. All children are under the perpetual care of some old women called *lā'tclās* who stay with them all the time. No one has ever been able to find out how many of these keepers there are and how they came to live in this underworld. The children are never visited by the other souls, as there are no means of communication between the two underworlds.

NEW YORK CITY.

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# OLD INDIAN GEOGRAPHICAL NAMES AROUND SANTA FE, NEW MEXICO

By J. P. HARRINGTON.

SEVERAL years ago the writer undertook, at the suggestion of Dr. Hewett, to collect the old Indian place names of the region about Santa Fe, New Mexico. Several hundred names of places were obtained by interviewing Indian and Spanish-speaking inhabitants and many of the localities were actually visited in company with one or more of the informants.[1] The present paper discusses in concise form the most important of these place names. Besides being of great local interest to the people of New Mexico, the place names throw certain light on the archaeology of the region and also on the subject of primitive geographical nomenclature in general.

## Abiquiu [3:36].

The original Tewa designation of this quaint Mexican town, which lies on the west bank of the Chama river eighteen miles above its confluence with the Rio Grande, is Phešu'u, literally "timber point" (phe, stick of wood, timber; šu'u, point of land, projecting point of hill or mesa, horizontally projecting end or point of any long object). What the name means is perfectly clear, yet why it was originally applied no Tewa knows. Either a former point of land with timber on it or a single projecting stick of timber was doubtless the originating landmark. The early Mexican colonists promptly corrupted Phešu'u into Abiquiu, the pronunciation of which does not differ as widely from the Tewa form as Spanish orthography might suggest. The present town stands almost on the site of the ancient pueblo, the Indian population of which gradually became Mexicanized and blended with the Mexican

[1] The results are published in the writer's paper entitled "The Ethnogeography of the Tewa Indians," *Twenty-ninth Annual Report of the Bureau of American Ethnology*, Washington, 1916, to which the reference numbers in brackets, given in the present paper, refer.



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settlers and with a considerable number of Indian captives, mostly of the Hopi tribe, who were settled there by the Mexicans. Because of these captives, Abiquiu has long been known also by a second name among the Tewa, namely Khoso'o$^{n}\eta$-'o$^{n}\eta$wi$^{n}$, Hopi town (Khoso'o$^{n}\eta$, Hopi Indian, literature a nickname, 'big leggings'; 'o$^{n}\eta$wi$^{n}$, pueblo).

Tewa traditions still tell of the time when great Indian fiestas were held at Abiquiu, attended by people from far and near. It was only a generation ago that the pa$^{n}\eta$šare, baile de los cautivos (pa$^{n}\eta$, captive; šare, dance) was discontinued there. This dance was given out of doors in the night time and Tewa, Mexican, and Hopi inhabitants participated. The Abiquiu of today, however, is merely a quaint old Mexican town which has lost both its Indian customs and Indian speech.

It follows from the descriptive nature of the nomenclature of places that two or more will frequently be found having the same name. It was by mere chance that the writer's San Ildefonso informants told of a second Phešu'u, a locality in the wild mesa lands south of San Ildefonso. This southern Phešu'u is a barren mesa top, seldom visited and known to few persons—a mute inglorious Hampton as compared with its famous namesake.

Abiquiu has been christened in Span. Santo Tomas de Abiquiu.

### Abiquiu Mountain [2:10]

This is the pointed peak twelve miles southwest of Abiquiu, 11,240 feet high according to Wheeler's measurements. A. F. Bandelier refers to it as "the pyramid of the extinct volcano of Abiquiu."[1] The Tewa name is Su$^{n}$p'i$^{n}\eta$, cicada mountain (šu$^{n}$, cicada; p'i$^{n}\eta$, mountain).

### Acoma [29:118]

The universal Keres name for the pueblo is Ak'o, of obscure etymology, first recorded by Father Marcos de Niza in 1539 as "Acus."[2] The form Acoma is from Keresan Ak'omæ, Acoma people (mæ, people).

[1] A. F. Bandelier, *Final Report*, pt. II, p. 32, 1892.

[2] Niça (1539) in Hakluyt, Voy., vol. III, p. 440, 1600.

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### ANCHO CANYON [17:62]

Ancho canyon is the next large canyon north of Frijoles canyon, and it, and not Frijoles canyon, is the bean canyon of the Tewa, the native form being Tunavahu'u (tu, bean; nava, field; hu'u, canyon). For the Tewa name of Frijoles canyon see below.

The latter part of this name happens to consist of the combination of nouns, navahu'u, field canyon, which probably gave rise to the tribal name Navajo.[1] Nava means a cultivated field, Span. siembra, and navahu'u, is applied to any arroyo or canyon where the people raise crops. There are many such arroyos in the rugged Navajo country, and it is likely that Navajo is a corruption of this descriptive Tewa term. The Tewa, however, have a distinct name for the Navajo, namely Wa$^{n}$nsave, Jemez Apache (Wa$^{n}\eta$, Jemez Indian; Save, Apache). The association of the Jemez and Navajo is well known.

### ARROYO HONDO [8:65]

The Arroyo Hondo near Taos is known to the Tewa as Ko$^{n}$-buts'i'i, barranco dell canyon (ko$^{n}$, barranco; bu'u, dell; ts'i'i, canyon). The Taos have for it a less patently descriptive name: P'ats'iyuhəaluna, water cicada arroyo (p'a, water; ts'iyu, cicada; həaluna, arroyo).

### BLACK MESA [18:19]

The great mesa of black basalt which stands a couple of miles north of San Ildefonso and which is believed by the Indians to have been anciently the house of a cannibal giant, is called in Tewa Thu$^{n}\eta$yo, very spotted (thu$^{n}\eta$, spotted; yo, augmentative). The name is old and the Indians are not sure why it was given, but informants have suggested that it was probably applied because of the great green blotches on the northern precipices of the mesa, above the giant's cave. It is by this cave that the giant used to enter the interior chambers of the mesa which were his dwelling place.

### BUCKMAN MESA [20:5]

Just as Thu$^{n}\eta$yo stands north of San Ildefonso, another gigantic black basalt mesa rears itself to the south of the village and almost

[1] See E. L. Hewett, *American Anthropologist*, N. S., vol. VIII, p. 193, 1906.



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equally distant. This southern rival of Thu$^{n}$ŋyo is called Šuma, a very old name the meaning of which has become forgotten. It is because of its situation between Thu$^{n}$ŋyo and Šuma that the Navajo have dubbed San Ildefonso picturesquely Tsĕ Tŭ Klnnĕ, houses between the rocks.[1]

CALLAMONGUE [21:24]

Callamongue is a Mexican hamlet on the east bank of Tesuque creek between Pojoaque and Tesuque. The old Tewa village ruin of K'uyemuge, from which the hamlet takes its name, lies half a mile west, across the creek. The ruin is on a mesa top and its name, which means 'where they hurled down stones' (k'u, stone; yemu, to throw pl.; ge, loc.), gives a glimpse of some defense in the long forgotten past.

CANGILON CREEK [1:31]

This eastern tributary of the Chama has a Tewa name equivalent in meaning to the Spanish: pæ$^{n}$se$^{n}$ŋhu'u, deer horn creek (pæ$^{n}$, deer; se$^{n}$ŋ, horn; hu'u, arroyo). In this and numerous similar instances, in which Indian and Spanish names are exact equivalents in meaning we have no means of determining whether the name was started by Spanish or Indian speakers.

CANOA MESA [13:1]

San Juan also has its great black basalt mesa, larger and higher than Thu$^{n}$ŋyo, though less imposing. This great block of basalt lies on the west side of the Rio Grande, north of the confluence with the Chama, and has its lower end opposite San Juan pueblo. To Mexican fancy it is shaped like a gigantic canoe and is known as the Mesa de la Canoa. The Tewa have for it however a very matter-of-fact name as compared with the old and mystical names Thu$^{n}$ŋyo and Šuma, namely: Tsi$^{n}$kwaye, basalt mesa (tsi$^{n}$, basalt; kwaye, height).

CAPULIN (p. 116)

Capulin, meaning in Spanish 'chokecherry,' has its Tewa counterpart: 'Ave'iwe, chokecherry place ('ave, chokecherry; 'iwe, loc.). Here again, we cannot determine which was the original.

[1] Curtis, *American Indian*, vol. 1, p. 138, 1907.

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### CERRO DE LOS BURROS [3:11]

The great mountain northwest of Abiquiu and across the Chama river from it, is called in Spanish Cerro de los Burros, for wild burros were formerly abundant there. The Tewa name on the other hand tells of the good piñones which were there gathered: T'omayop-'i$^{n}\eta$, good piñon mountain (t'o, piñon; mayo, excellent; p'i$^{n}\eta$, mountain).

### CHAMA [5:7]

The writer was guided by San Juan Indians to the old pueblo ruin of Tsa$^{n}$ma$^{n}$, which has given the name to both the Chama river and to Chamita hamlet. Tsa$^{n}$ma$^{n}$ ruin occupies a low mesa on the eastern bank of the Chama river, a mile and a half southeast of the mouth of El Rito creek and fully ten miles northwest of its linguistic offspring Chamita. The name is said to mean 'where they wrestled' (tsa$^{n}$ma$^{n}$, to have wrestled). Tsa$^{n}$ma$^{n}$ must once have been an important pueblo, but had been forgotten until it was known to no whites and to but few Tewa, although its name lived on, its origin quite unknown, generalized to cover the whole Chama river, and again in specialized Spanish diminutive form as Chamita. Father Zárate-Salmeron writes Zama,[1] the other early records all show Chama. Apparently at the time of these records the name had already become extended in Spanish to apply to the whole Chama region and river.

### CHAMA RIVER (Large Features :2]

But among the Tewa Tsa$^{n}$ma$^{n}$ is applied only to the locality of the pueblo ruin. The Chama river is in Tewa P'op'i$^{n}\eta$, red river (p'o, water, river; p'i$^{n}\eta$ for p'i'i$^{n}\eta$: p'i, red; 'i$^{n}\eta$, gender postfix). The Rio Grande is frequently red for miles below the confluence because of the water discharged by the Chama. Bandelier learned that the Chama in turn gets its red water from Coyote creek [1:29]: "The branches of which the Chama is formed are the Coyote [1:29] in the west, the Gallinas [1:24] north of west, and the Nutrias [1:14] north. It is said that the waters of the first are red, those of the Gallinas white, and those of the Nutrias limpid. According as one

[1] Quoted by Bandelier, *Final Report*, pt. II, p. 60, 1892.

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or the other of these tributaries rises, the waters of the Chama assume a different hue.[1]"

The Keres language is especially fond of naming places from cardinal directions. Among the Cochiteños, the Chama is T$^{y}$ete-potšina, northwest river (t$^{y}$ete, north; po, west; tšina, river).

### Chilí [5:46]

Chilí, a tiny Mexican settlement on the west side of the Chama just below the mouth of Ojo Caliente creek, has Tewa and Span. names equally obscure. The Tewa name, Tsip'apu, appears to contain tsi'i, flint, and pu, buttocks. Neither Mexicans nor Tewa know the source of the name Chilí.

### Chimayó [22:18]

The famous Chimayó blankets have caused the name Chimayó to be circulated more widely than perhaps any other of Tewa origin. They are woven by Mexicans at Chimayó hamlet in the Cañada de Santa Cruz. The Tewa form is Tsimayó, good obsidian (tsi'i, obsidian; mayo, excellent). The church at the hamlet, which is, by the way, famous as a shrine, stands on the site of the long vanished pueblo of Tsimayó.

### Chipiwí [14:39]

Chipiwí, a ruined pueblo southwest of Puyé, is in Tewa Tsipi-wi'i, gap where the obsidian comes out (of the ground) (tsi'i, obsidian; pi, to come out; wi'i, gap).

### Cochiti [28:77]

The native Keres name is K'ot'$^{y}$iti, obscure in meaning. This the Tewa have borrowed and folk-etymologized into K'ute'e, stone kiva (k'u, stone; te'e, kiva), although of course there is no stone kiva at Cochiti; the Jemez into K$^{y}$atəge, mountain-sheep pueblo (k$^{y}$a, mountain-sheep; təge, pueblo).

### Corral de Piedra [14:15]

For this hamlet, north of Española, the Tewa and Spanish names mean the same: Tewa K'utepa'iwe, stone wall place (k'u, stone; tepa, wall; 'iwe, loc.).

[1] Bandelier, *Final Report*, pt. II, p. 55, 1892.

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### CUNDAYÓ [25:8]

The Tewa pueblo ruin Kudiyo (obscure: yo apparently augmentative) lies on the mesa southwest of Cundayó hamlet. The locality is several miles northeast of Nambé.

### EL RITO [4:4]

The El Rito region is called in Tewa P'i'a$^{n}$nuge, pink below place (p'i'a$^{n}$, pink, from p'i, red, and *'a*$^{n}$, brown; nu'u, below; ge, loc.), referring to the El Rito mountains, p'i'a$^{n}$p'i$^{n}\eta$, pink mountains (p'i$^{n}\eta$, mountain).

### EMBUDO [8:73]

Embudo town and canyon, above San Juan, are named by the Tewa Pore'iwe, fishweir place (pore, fishweir; 'iwe, place). Span. Embudo, funnel, is a descriptive name applied to the canyon.

### ESPAÑOLA [14:16]

Española, literally, 'Spanish town,' is called by the Tewa Butsa$^{n}$bi'i, new town (bu'u, plaza, town; tsa$^{n}$bi'i, new inan.).

### ESTACA [10:4]

Estaca settlement is on the west side of the Rio Grande, at the foot of Canoa Mesa, north of San Juan. The Tewa call Estaca Na$^{n}$mphonu'u, below where the holes are in the ground; or the place below, where the holes are in the ground (na$^{n}\eta$, earth; pho, hole; nu'u, below).

### FRIJOLES CANYON [28:6]

The Rito de los Frijoles is in Tewa Puqwige, where they scraped or wiped the bottoms (possibly of the pottery vessels) (pu, buttocks, bottom; qwi, to scrape, to wipe; ge, loc.). The Tewa name is difficult to analyse; and T$^{y}$u'on$^{y}$i, the Cochiti name, does not yield at all to analysis.

### GALISTEO [29:39]

Galisteo pueblo ruin and modern town are the Thanuge *par excellence* of the Tewa, although this name is also applied to the whole region south of Santa Fe—the old Tano country. Thanuge means live down-country place (tha, to dwell; nu'u, below; ge, loc.).

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GALLINAS [1:24]

Tewa Dip'o, chicken creek (di, chicken; p'o, water) is perhaps a mere translation of the Span. name. Di in primitive Tewa meant turkey, but became so familiar a word when applied to the introduced gallinas of the Mexicans that turkey is now designated by the compound p'i$^{n}$ndi, mountain turkey (p'i$^{n}\eta$, mountain), or if you will, mountain chicken. Therefore an original Tewa place name dip'o would have become in Spanish Los Guajalotes, not Las Gallinas.

GAVILAN [7:3]

In the case of the name Gavilan, a Mexican settlement on Ojo Caliente creek, however, we have perhaps a clew to show that the Tewa name was. The original Span. gavilan means any kind of hawk; the Tewa name of the place, tšugæ$^{n}$'iwe, means place of a certain species of hawk, Falco nisus (tšugæ$^{n}$, Falco nisus;-'iwe, loc.), the Tewa having no general term for hawk. The Spanish name therefore in this case apparently presupposes the more definite or peculiar Tewa name.

GUACHE [14:11]

Guache, a Mexican hamlet north of Española, is called in Tewa ma$^{n}$hu$^{n}$bu'u, owl dell (ma$^{n}$hu$^{n}$, great horned owl; bu'u, dell). Guache sounds indeed like a loan-word from the Tewa (cf.Guachepanque), but no Tewa counterpart is discoverable.

GUACHEPANQUE [14:20]

Guachepanque, a Mexican settlement between Española and Santa Clara, is in Tewa P'otsip'a$^{n'an}$ge, mud string place (p'otsi, mud; p'a$^{n'an}$, string; ge, place). Could the original idea have been a string of mudpuddles?

GUAJE CANYON [16:53]

This Spanish name, which means gourd canyon, is not reflected in Tewa. To the Tewa the canyon is simply ts'iso'o, the big canyon (ts'i'i, canyon; so'o, big).

HOPI VILLAGES [Unmapped]

Tusayan is called by the Tewa: Khoso$^{n}$'o$^{n}\eta$'o$^{n}\eta$wi$^{n}$, Hopi villages (Khoso$^{n'on}\eta$, Hopi Indian, lit., big leggings: kho, leggings; so$^{n}$'o$^{n}\eta$, big *veg.*; 'o$^{n}\eta$wi$^{n}$, pueblo).

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### ISLETA [29:101]

Tewa Tsiqwevege, kick flint place (tsi'i, flint; qweve, to kick along; ge, loc.), evidently means where they played the kicking-race (the game called gome in Span.) using a piece of obsidian. The native Isleta name, Šiahwibak is doubtless cognate.

### JACONA [21:6]

This settlement is a mile west of Poquaque and its name is a corruption of the old Tewa name of the locality: Sako$^{n}$næ$^{n}$, tobacco bank place (sa, tobacco; ko$^{n}$, barranco; næ$^{n}$, loc.). Jacona is also a sign-board station on the Denver and Rio Grande railway between Santa Fe and Buckman, miles to the south of the real Jacona.

### JEMEZ [27:35]

The Tewa have a special name for Jemez, namely Wa$^{n}\eta$ge, Jemez Indian place (Wa$^{n}\eta$, Jemez Indian, unexplained; ge, loc.). The Jemez Indians' own name for the pueblo is He$^{n}$wa, He$^{n}$kwa or He$^{n}$yo, according as various locative postfixes are used, this being derived from the term for Jemez person: He$^{n}$, pl. He$^{n}$miš. It is from the pl. form, meaning Jemez people, that Span. Jemez, Ker. Hæmiši, etc., are derived. It is possible that Jemez He$^{n}$, Jemez Indian, is cognate with Tewa Wa$^{n}\eta$, of same meaning.

### JEMEZ MOUNTAINS [Large Features :8]

The rather inappropriately named Jemez mountains, the range that bounds the Tewa country on the west, are referred to in Tewa merely as Tsa$^{n}$mpiye'i'$^{l}$p'i$^{n}\eta$, the western mountains (tsa$^{n}$mpiye west; 'i'$^{l}$, gender postfix; p'i$^{n}\eta$, mountain).

### JICARILLA MOUNTAIN [22:9]

Tewa T'u$^{n}$mp'i$^{n}\eta$, basket mountain (t'u$^{n}\eta$, basket; p'i$^{n}\eta$, mountain). The peak is thought to resemble an inverted basket; hence both Span. and Tewa name.

### LA CUEVA [6:28]

La Cueva, on Ojo Caliente creek above Ojo Caliente, has a Tewa name, Ma$^{n}$hu$^{n}$wiri, owl point (ma$^{n}$hu$^{n}$, owl; wiri, point), which presupposes or is derived from Ma$^{n}$hu$^{n}$se$^{n}$nnæ$^{n}$ [6:6], owl's

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horns (ma$^{n}$hu$^{n}$, owl; se$^{n}$$\eta$, horn; næ$^{n}$, loc.), the name of the great mountain east of La Cueva. The mountain has two peaks resembling the horns of ma$^{n}$hu$^{n}$, great horned owl.

### La Joya [9:5]

The locality of La Joya, at the confluence of Truchas creek with the Rio Grande, the Spanish name of which means merely 'the dell,' is called in Tewa Tsigubu'u, chico dell (tsigu, an unidentified bush sp.; bu'u, dell). The bush is the 'chico' of the local Mexicans, a name which is probably a corruption of the Tewa name.

### Laguna [29:117]

The Tewa have a merely descriptive term meaning the same as the Spanish name: P'okwi$^{n}$diwe, lake place (p'okwi$^{n}$i, lake; 'iwe. loc). The Keres name, K'awaik'a, however, does not yield to etymology.

### Lake Peak [22:54]

Lake Peak of the Santa Fe range including its lake, which is most sacred to the Tewa, is called Agatšænup'i$^{n}$$\eta$ (obscure: p'i$^{n}$$\eta$, mountain). This peak is the Tewa sacred mountain of the east.

### Lamy Canyon [29:37]

Lamy and the canyon above Lamy, up which the Santa Fe railroad passes, are called in Tewa Pi$^{n}$mp'oyehu'u, heart water meet canyon (pi$^{n}$$\eta$, heart; p'o, water; ye, to meet; hu'u, arroyo, canyon). The exact force of the name is obscure.

### Nambé [23:5]

Tewa Na$^{n}$mbe'e, roundish earth (na$^{n}$$\eta$, earth; be'e, roundish and small), was probably originally applied because of a mound of earth. The name was transferred to the present site when the village was moved thither from old Na$^{n}$mbe'e pueblo ruin [25:30], which lies in the mountains several miles northeast of the present Nambé.

### Navawí [16:74] [17:15]

Tewa Navawi'i means pitfall gap (nava, pitfall; wi'i, gap). There are two places by this name, [16:74] and [17:15], both situated

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in the Pajarito plateau, across the Rio Grande southwest of San Ildefonso. These pitfalls were bottle-shaped excavations in the tufaceous ground made in narrow gaps or on trails where game was wont to pass and deer and other game were caught in them.

### Ojo Caliente [6:24]

Tewa P'osi-, emerald-green (absolute form of the adjective p'osiwi'$^{l}$, p'osiwi$^{n}\eta$) is the old name of Ojo Caliente. The hotsprings cover the rock with an emerald green stain; hence the name. The pueblo ruin at Ojo Caliente, P'osi'o$^{n}\eta$wi$^{n}$, emerald green pueblo ('o$^{n}\eta$wi$^{n}$, pueblo) was once the most important village of the Tewa if we can trust Tewa tradition, and it is said that at that pueblo P'oseyemu, the Tewa culture hero, was born of a virgin, grew up and at last revealed himself to the people. The old pool, over which the bathhouse is now built, was a most sacred place to the Tewa; P'oseyemu used to enter or emerge from that pool at times; pools and lakelets are regarded as doorways to the nether world.

### Oso Creek [5:35]

The Tewa name is Pheserep'o, shove stick creek (phe, stick of wood; sere, to shove; p'o, water). Phesere is also the name of a pueblo ruin [5:37] which lies on the southern bank of the creek and it is not unlikely that the creek takes its name from the ruin.

### Otowí [16:105]

One of the largest ruins of the Pajarito plateau is the P'otsuwi'i of the Tewa, gap where the water sinks (p'o, water; tsu, to enter; wi'i, gap). That the ruin lies at a gap or pass is well known; it will be interesting to investigate whether a stream or spring ever sinks into the ground anywhere at the locality at the present day and age.

### Painted Cave [25:31]

The great painted cave of the Pajarito plateau with its ancient paintings is known both to the Tewa and the Keres by purely descriptive names: Tewa T'ovaqwata$^{n'an}$di'$^{l}$, painted cave (t'-ovaqwa, cave; ta$^{n'an}$, painted; 'i',$^{l}$ gender postfix); Coch. Tset$^{y}$-ateṭanš-k'athet$^{y}$amą, painted cave (tset$^{y}$ateṭanš, painted; k'athet$^{y}$-

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amą, cave). I fear that both these terms look equally frightful to the novice. There is no reason to suppose that the Tewa and Cochiti forms are not the pristine names of the place, despite their simple descriptive meaning.

PECOS [29:33]

Three different names for Pecos are found in the Pueblo languages.

1. The native Pecos name K'ak'ora, where the stone is on top (k'a, stone; k'o, to be on top; ra, loc.). It is from a Tiwa form cognate with this Pecos name (see Picuris Hiuqūā, Isleta. Hiokŭö-, etc., quoted under [29:33]; Tiwa hiu- = Pecos k'a, stone) that Cicuye, in its various spellings, is derived.

2. Tewa Tšu$^{n}$ŋge, place of an unidentified bush sp. (tšu$^{n}$ŋ, plant sp.; ge, loc.).

3. Jemez P'ak$^{y}$ula (obscure: p'a, water). Coch. Pæyokhona (obscure). It is apparently from the Ker. form that Span. Pecos (a pl. form used in the sing., cf. the name Taos) is derived.

PEDERNAL PEAK [2:9]

Although Span., Tewa, and Cochiti, names all mean obsidian mountain, informants who had visited the mountain volunteered that obsidian is not more plentiful there than at other mountains of the western range. The Tewa form is Tsip'i$^{n}$ŋ (tsi'i, obsidian; p'i$^{n}$ŋ, mountain); the Cochiti Heṣt'e'yan$^{y}$ik'othe (heṣt'e'yan$^{y}$i, obsidian; k'othe, mountain). The peak lies south of Abiquiu and its truncated cone is far visible and a prominent landmark.

PICURIS [8:88]

The Tewa and Jemez, forms mean mountain gap, mountain pass: Tewa P'i$^{n}$ŋwi'i (p'i$^{n}$ŋ, mountain; wi'i, gap); Jemez P'ekwile- (p'e, mountain; kwile-, pass). From some such form as the Jemez comes Span. Picuris (originally a Span. pl. form).

PLACITA LARGA [14:12]

The Span. name means 'long plaza.' The Tewa 'O$^{n}$wi$^{n}$heyi or Buheyi ('o$^{n}$wi$^{n}$, town; heyi, long; bu'u, plaza, town) doubtless merely follows the Spanish.

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### POJOAQUE [21:29]

Span. Pojoaque is for Tewa P'osu$^{n}\eta$wæ$^{n}$ge, drink water place (p'o, water; su$^{n}\eta$wæ$^{n}$, to drink; *ge*, loc.). This once populous Tewa pueblo is now practically extinct.

### PUEBLITO [13:15]

Pueblito is a small Indian suburb of San Juan on the west side of the Rio Grande. It is known to the Tewa as K'un$^{y}$æ$^{n}$'o$^{n}\eta$wi$^{n}$, turquoise pueblo (k'un$^{y}$æ$^{n}$, turquoise; 'o$^{n}\eta$wi$^{n}$, pueblo), a name which happens to be applied also to San Marcos pueblo ruin, south of Santa Fe (see below).

### PUEBLO PEAK [8:40]

This is the high peak east of Taos. The Taos call it Ma$^{n}$-qwaluna$^{n}$ (obscure), which the Tewa corrupt into Ma$^{n}$qwolop'i$^{n}\eta$ (p'i$^{n}\eta$, mountain).

### PUYÉ [14:46]

The old Santa Clara Tewa name is Puye, apparently meaning 'where the cottontail rabbits assemble' (pu, cottontail rabbit; ye, to meet, to assemble).

### RIO GRANDE [Large Features :3]

Most of the Pueblo names for the Rio Grande mean 'big river,' just as the Span. name does, but this does not preclude their being ancient names. Thus Tewa P'osoge (p'o, water; so'o, big; *ge*, loc.); Jemez Ha$^{n}$n$^{y}$ap'akwa (ha$^{n}$n$^{y}$a, big; p'a, water; kwa, loc.). The Cochiti however call it merely Tšina, the river (tšina, river).

### SAN FELIPE [29:69]

The Tewa name of San Felipe is Na$^{n}\eta$kwæ$^{n}$rige, sticky earth place (na$^{n}\eta$, earth; kwæri, sticky; *ge*, loc.); the Jemez Kwilegi'i, apparently 'gap place' (kwile, gap; gi'i, loc.). The Keres has an unanalysable name: Katṣt$^{y}$a.

### SAN GABRIEL [13:27]

This old pueblo ruin, on the west side of the Rio Grande opposite San Juan, has the Tewa name Yu$^{n}\eta$ge, apparently meaning

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'mockingbird place' (yu$^{n}\eta$, mockingbird; *ge*, loc.). Bandelier's "Yuge-uingge"[1] is for Yu$^{n}\eta$ge'o$^{n}\eta$wi$^{n}$ge ('o$^{n}\eta$wi$^{n}$, pueblo; *ge*, loc.).

### San Ildefonso [19:22]

The Tewa name is P'oqwoge, where the water cut through (p'o, water; qwo, to cut through; *ge*, loc.). But where it cut through or under what circumstances can never be recovered from the long forgotten past. Jemez P'ašugi'i (gi'i, loc.) and Coch. P'akhwete are clearly forms of the same name.

### San Juan [11: San Juan Pueblo]

San Juan is known to the Tewa as 'Oke. The meaning is unknown, but there is nothing in the phonetics of the word to prevent it meaning hard metate ('o, metate; ke, hard). The present 'Oke is the third site by that name. Old 'Oke [10:26] is a mile northwest of the present village. The legend goes that an Indian of 'Oke, while taking a twelve day ceremonial fast, became so crazed for water that he broke from his confinement, and rushing to a swamp near the river drank until he burst. The water from his body flooded the pueblo and destroyed it. The inhabitants fled and founded a second 'Oke at [11:17], in the lowlands just north of the present pueblo. From there the pueblo gradually shifted to the high ground where it now stands.

### San Marcos [29: unlocated]

This old pueblo ruin of the Tano tribe, south of Santa Fe, the Tewa call K'un$^{y}$æ$^{n}$'o$^{n}\eta$wi$^{n}$, turquoise pueblo. Pueblito has this same name; see above.

### Sandia [29:100]

The Sandia name is Na$^{n}$Fi$^{n}$a$\theta$ ($\theta$, loc.), the Cochiti name Waṣetsæ (–tsæ, loc.); both these names are obscure.

### Sandia Mountain [29:83]

This mountain is perhaps the most prominent geographical feature of central New Mexico. It is mentioned in Pueblo mythology and is the sacred mountain of the south of the Tewa, who call

[1] Bandelier, *Final Report*, pt. II, p. 48 *et passim*, 1892.

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it 'Ōkūp'i$^{n}\eta$, turtle mountain ('ōkū, turtle; p'i$^{n}\eta$, mountain). Jemez Kiutawe and Cochiti Tsepe are obscure.

### SANTA ANA [29:95]

The Tewa name, Šarege, means dancing place (šare, to dance; ge, loc.). Jemez Tu$^{n}$dagi'i and Keres Tamaya are obscure.

### SANTA CLARA [14:71]

The Tewa name, Khap'o, is obscure in meaning (kha, corral, heavy, rose, spherical; p'o, water, trail). Some Indians have suggested pretty folk-etymologies by arbitrarily preferring certain meanings for kha and p'o. Jemez Š$^{y}$ap'agi'i is evidently the same word as Khap'o plus the Jemez loc. gi'i. Keres shows the form Kaip'a.

### SANTA CLARA PEAK [2:13]

This loftiest peak of the Jemez range, 11,260 feet high, lies west of Santa Clara pueblo and has been called Santa Clara peak. It is the sacred mountain of the west of the Tewa, the sacred mountain of the east of the Navajo. The Tewa name, Tsiku'mup'i$^{n}\eta$, almost surely means 'obsidian-covered mountain,' *i.e.*, 'mountain that is covered with pieces of obsidian' (tsi'i, obsidian; ku'mu, to be covered; p'i$^{n}\eta$, mountain).

### SANTA CRUZ [15:19]

Santa Cruz is called in Tewa, evidently because of the much mentioned Cañada de Santa Cruz, Kan$^{y}$æ$^{n}$ra'i$^{n}$mbu'u, Cañada town (kan$^{y}$æ$^{n}$ra, cañada, from the Span.; 'i$^{n}\eta$, gender postfix; bu'u, town).

### SANTA FE [29:5]

The general Tewa name for the city, also the creek and whole locality of Santa Fe, is 'Ogap'oge, olivella water place ('oga, olivella shell; p'o, water; ge, loc.). The Tewa knew the olivella in ancient times and prized it for making shell-money; the shells came from the far distant Gulf of California and the California coast, being bartered from tribe to tribe until they reached the Tewa, more than a thousand miles inland. San Juan Tewa shows a variant form

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of the name: Kwa'ap'oge, bead water place (kwa'a, any bead), a name which has practically the same meaning as the name current at the other Tewa pueblos. Cochiti Keres with characteristic fondness for directional naming disposes of Santa Fe as Hašok'o, east corner (ha, east; šok'o, corner, dell).

SANTA FE BALDY [22:53]

This is the Povip'i$^{n}\eta$, flower mountain, of the Tewa (povi, flower; p'i$^{n}\eta$, mountain). Santa Fe Baldy is one of the numerous Cerros Pelados, bald mountains, of New Mexican Spanish nomenclature. Its high and bald peak is conspicuous in the Santa Fe range.

SANTA FE MOUNTAINS [Large Features :1]

The Santa Fe mountains are the Tha$^{n}$mpiye'i$^{l}$p'i$^{n}\eta$, 'eastern mountains' of the Tewa (tha$^{n}$mpiye, east; 'i$^{l}$, gender postfix; p'i$^{n}\eta$, mountain).

SANTO DOMINGO [28:109]

The old Keres name occurs in Cochiti as T$^{y}$e'wa, in S. Dom. as K$^{y}$e'wa and is obscure in etymology. It has been borrowed by Tewa as Tewige, by Jemez as Tawigi'i (ge, gi'i, loc.).

SIA [29:94]

Tewa has a descriptive term, 'Okuwarege, scattered hills place ('oku, hill; ware, to be scattered; ge, loc.). The old and obscure Keres form is Tse'ya, whence Jemez Sa'yakwa (kwa, loc.).

STONE LIONS SHRINE [28:27]

Tewa, Jemez, and Cochiti names are purely descriptive; there is no reason to suppose, however, that they are not the ancient designations.

1. Tewa Khæ$^{n}$da'æ$^{n}$ndiwe, where the two mountain-lions sit (khæ$^{n}\eta$, mountain-lion; ra-, they two; 'æ$^{n}\eta$, to sit; 'iwe, loc.).

2. Jemez Š$^{y}$at$^{y}$esi$^{n}$lenu$^{n}$, where the mountain-lions sit (š$^{y}$at$^{y}$e, mountain-lion; ši$^{n}$le, to sit; nu$^{n}$, loc.).

3. Cochiti Mok'aṭąk'owetæ, where the two mountain-lions lie (mok'aṭą, mountain-lion; k'owe, to lie; tæ, loc.).

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### TAOS [8:45]

The Taos name for Taos is Təa-, whence doubtless Span. Taos (originally a pl. form) and Tewa Thawi'i, folk-etymologized to sound as if it meant 'gap or pass where they live' (tha, to dwell; wi'i, gap). Taos Təa- belongs to a family of words which appear in the various Tanoan languages with the meanings to dwell, house, pueblo.

A second Taos name for Taos is 'Ialaphai-, red-willow trees ('ia, willow; ła, wood, tree; phai, red). This has a counterpart in the Jem. name for Taos: Yu'la-.

The Cochiti Keres language again characteristically disposes of Taos as $T^{y}et^{y}$šok'otsæ, north corner place ($t^{y}et^{y}$, north; šok'o, corner, dell; tsæ, loc.).

### TSIREGE [17:34]

The name of this pueblo ruin, which has given the name to the Pajarito plateau, is in Tewa Tsirege, bird place (tsire, any bird; ge, loc.). Cochiti Waṣteṭ-, bird, and Span. El Pajarito, have equivalent meaning.

### TESUQUE [26:8]

Tesuque is from Tewa T'athu$^{n}\eta$ge, dry spotted place (t'a, dry; thu$^{n}\eta$, spotted; ge, loc.).

### TIERRA AMARILLA [1:12]

A little northeast of Tierra Amarilla town is a deposit of yellow earth which was known to the ancient Tewa and was used by them for yellowing the interior walls of houses. This earth is called in Tewa na$^{n}$nts'eyi'$^{l}$ (na'$^{n}\eta$, earth; ts'eyi'$^{l}$, ts'eyi$^{n}\eta$, yellow) and gives the name to the town and region: Na$^{n}$nts'eyiwe ('iwe, loc.).

### TIERRA AZUL [3:26]

The Tewa call Tierra Azul settlement, which lies on the south side of the Chama below Abiquiu, Na$^{n}$ntsa$^{n}\eta$wæ$^{n}$bu'u, blue earth dell or town (na$^{n}\eta$, earth; tsa$^{n}\eta$wæ$^{n}$, blue; bu'u, dell, also plaza, town). The ground is bluish or rather grayish in the locality. The Tewa name is probably old.

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TRES PIEDRAS [8:12]

This town, which lies west of the Rio Grande and northwest of Taos, is called in Taos and Tewa 'mountain-sheep rocks': Taos Kuwahiuna (kuwa, mountain-sheep; hiuna, stone, rock); Tewa Kuwak'u (kuwa, mountain-sheep; k'u, stone, rock).

TRUCHAS CREEK [9:9]

Truchas creek, eastern tributary of the Rio Grande, is called in Tewa: 'Omæⁿηge'iⁿηhu'u (obscure: 'iⁿη, gender postfix; hu'u, arroyo).

TRUCHAS PEAK [22:13]

Truchas peak of the Santa Fe range is the Tewa K'useⁿmp'iⁿη, rock horn mountain (k'u, stone; seⁿη, horn; p'iⁿη, mountain). It is curious that Bandelier, knowing nothing of the Tewa name of Truchas peak or its meaning, writes: "The summit of the Truchas is divided into sharp-pointed peaks, recalling the 'Hörner Stöcke' or 'Dents' of the Alps."[1]

TSANKAWI [16:114]

Tewa Sæᵖk'ewi'i, sharp cactus gap (sæⁿ, opuntia, leaf-cactus; k'e, sharp; wi'i, gap) is the name of the pueblo ruin and the gap where it stands. Sæⁿk'ewi'i is in the Pajarito Plateau.

TSAWARI [15:24]

Tewa Ts'æⁿwari, white wide gap (ts'æⁿ, white; wori, wide gap) is a place in the Cañada de Santa Cruz four miles above its mouth. A broad stratum or belt of soft whitish rock crosses the Cañada there. On the mesa on the south side of the Cañada lies the pueblo ruin, and the Mexican hamlet of Puebla adjoins the ruin on the west. This ruin was a village of the Tano Indians. It was built by them after they left their ancient home in the Galisteo region, and was abandoned in 1696 when they migrated to the Hopi country. The Tewa have heard that the people of Ts'æⁿwari fled to the Hopi to escape from the tyranny of the Mexicans and to help the Hopi fight the Navajo and the Mexicans.

[1] Bandelier, *Final Report*, pt. II, p. 35, 1892.

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When the people left Ts'æⁿwari they buried a large storage-jar, Span. tinajon, filled with blue turquoise, red coral and other beautiful possessions somewhere near the pueblo. What the jar contains is very valuable; many have dug for it but no one has found it.

### TURQUOISE MINES [29:55]

The famous Turquoise mines south of Santa Fe are called in Tewa K'unʸæⁿ'iwe, turquoise place (k'unʸæⁿ, turquoise;-'iwe, loc.). Turquoise was dug there in pre-Columbian times by Tewa and Keres Indians.

### ZUÑI [Unmapped]

Tewa Sunʸi- and Jem. Sənigi'i (gi'i, loc.) are perhaps from Ker. Sunʸi. It is likely that Span. Zuñi also is from the Keres.

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# A MAYA ACCOUNT OF THE CREATION

By RALPH L. ROYS

AS creation myths are always of more than ordinary interest I wish to draw attention to the following account of the creation of the world according to the conception of the Maya Indians of Yucatan. It is found on plates 60, 61 and 62 of the Book of Chilam Balam of Chumayel (University of Pennsylvania. The Museum. *Anthropological Publications*, vol. v).

This book dates only from the latter half of the eighteenth century but it is a compilation of extracts from earlier Maya books which were also probably known as Books of Chilam Balam. It contains several historical accounts of the periods both before and after the Spanish conquest of Yucatan, a great many prophecies and a number of mythological stories.

Three of these historical accounts and several of the prophecies together with translations appear in Brinton's Maya Chronicles and a number of the prophecies are to be found in various histories of Yucatan, but only one of the mythological stories has been published and translated as far as I know. This is the contribution of D. Juan Martinez y Hernandez to the International Congress of Americanists of 1912 and is entitled "La Creación del Mundo segun los mayas. Páginas inéditas del M.S. de Chumayel." In this paper he has translated a passage from the manuscript which is to be found on plates 42, 43 and 44 of the University of the Pennsylvania reproduction and although, as the title indicates, this is a creation story, it does not resemble the one which follows.

Christian influence has affected the form of the following account to some extent but not the content and the most interesting feature is the apparent reference to the event pictured on page 74 of the Dresden Codex. It would seem certain that sooner or later some parallel passages must be found to occur in the Books of Chilam Balam and the Maya picture manuscripts.



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## TEXT [1]

Bay tzolci yax ah miatz. merchise: yax ah bouat: Napuctun. sacerdote. yax ah kin lay kay uchci u cihil uinal. ti ma to ahaccab cuchie. ca hoppi u ximbal tu ba tu hunal = ca yalah u chich: ca yalah u ɔenaa: ca yalah u mim: ca yalah u muu: Bal bin c alab. ca bin c ylab uinic ti be: Cu thanob tamuk u ximbalob: cuchie: minan uinic cuchi: Catun kuchiob te ti likine: Ca hoppi yalicob: Mac ti mani uay lae: he yocob lae: Ppis tau oci: Ci bin u than u colel cab. Ca bin u ppisah yoc ca yumil ti $D^s$. citbil. lay u chun yalci. Xoc lahcab: oc lae. lahca oc = Lay tzolan cihi. tumen Oxlahun Oc = uchci u nup tanba yoc: Likciob te ti likine: Ca yalah u kaba = ti minan u kaba kin cuchie: ximbalnahci: *y* u chiich: *y* u enaa: *y* u mim: *y* u muu: Ci uinal: çihci kin = u kaba çihci = caan *y* luum: eb haa: luum: tunich: *y* che: çihci u bal kaknab: y luum:

Hun Chuen. u kokçici uba tu kuil: u mentci caan *y* luum:

Ca Eb: u mentci yax $eb^{2,3}$ = emci likul tanyollcaan: tanyol haa: minan luum *y* tunich: *y* che:

Ox Men (Ben): u mentci tulacal bal. hibahun. bal: u bal caanob *y* u bal kaknab *y* u bal luum

Can Ix: uchci u nixpahal caan *y* luum:

Ho Men = uchci u meyah tulacal.

Uac Cib. uchci u mentci: yax cib. uchci u çasilhal: ti minankin: *y u*:

Uac (Uuc) Caban. yax çihci cab: ti minan toon cuchi:

Uaxac Eɔnab: eɔlahci. u kab. *y* yoc: Ca u chichaah (chichah), yokol luum:

Bolon Cauac: yax tumtabci: metnal:

Lahun Ahau: uchci u binob: u lobil uinicob ti metnal tumen $D^s$ citbil: ma chicanac cuchie:

Bulu(c) (Ym)yx uchci u patic tunich *y* che: lay u mentah ychil kin =

Lahcabil Yk: uchci u çihçic yk. = Lay u chun u kabatic yk. tumen minan cimil ychil lae:

Oxlahun Ak(b)al uchci u chaic haa = ca yoksah luum = ca u patah ca uinic hi:

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Hunnil Kan = u yax mentci u leppel yol tumenel u lobil u çihçah =

Ca Chicchan: uchci u chictahal u lobil hibal yilah ychil u uich cahe =

Ox Cimil u tusci cimil uchci. u tusci yax cimil: ca yumil ti Ds. (Can Manik)

Ho Lamat: lay u tusci uuclam chac kaal kaknab =

Uac Muluc, uchci u mucchahal kopob tulacal: ti ma to ahac cabe: lay uchci yocol u tusthanil ca yumil ti D$^s$ tulacal = ti minan tun than ti caan: ti minan tunich *y* che: cuchi:

Catun binob u tumtubaob ca yalah tun bayla = oxlahun tuc[4]::: uuc tuc hun = lay yalah: ca hok u than ti minan than ti:: ca katab u chun tumen yax ahau-kin maix hepahac u nucul than tiob: uchebal u thanic ubaobe:: ca binob tan yol caan ca u machaah u kab tuba tanbaobe: Catun ualah tan chumuc peten keklayob lae:: Heklayob ah toocob cantulob lae[5] =

.....

Can Chicchan.....O Ah toc E
Canil.....Oc Ah toc Pio
Ca(n) Men.....O Ah toc MER
Can Ahau.....O Ah toc
.....lay ahauob cantulob lae.....

| | | |
|---|---|---|
| Uaxac Muluc | | Hoil Cauac |
| Bolon Oc | | Uac Ahau |
| Lahun Chuen | 2 | Uay (Uuc) Ymix |
| Buluc Eb | | Uaxacil Yk |
| Lahca Men (Ben) | 4 | Bolon Akabal |
| Oxlahun Yx | 5 | Lahun Kan |
| Hun Men | 6 | Buluc Chicchan |
| Ca Cib | | Lahca Cimity |
| Ox Caban | 7 | Oxlahun Manik |
| Can Eɔnab | | Hun Lamat |

Lay çihi uinal *y* uchci yahal cab: tzolci caan *y* luum. *y* cheob *y* tunich. Cihi tulacal tumen ca yumil ti D$^s$ lae Lay cubil ti minan caan *y* luum: ti bay yanil tu Diosil tu muyalil tuba tu hunal Ca u çihcah (çihçah) balcah tuçinil: Ca pecnahi tu caanil tu kuil ti bay noh u chucil yanil ah tepale.

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U tzolan kin çançamal licil u xocol u chun ti likine hebix tzolanile.

TRANSLATION

Thus it was set in order by the first wise man, merchise, the first prophet, Napuctun, sacerdote, the first priest. This is the song of how the creation of the uinal came to pass when the world was not yet created. Then he began to march by his own force. And then said his mother's mother, then said his mother's sister, then said his father's mother (or mother's grandmother), then said his sister-in-law, "How shall we make manifest and see man upon the road?" These were their words while they marched, but there was no man then. Then they arrived there in the east and began to speak: "Who has passed here? Here are footsteps; measure it off with your feet." These were the words of the mistress of the world. Then our Lord, Dios supreme, measured it off with his feet. This is what he first said: "The count is twelve paces, twelve paces." This was set in order by Oxlahun Oc. His feet came up even with one another. They departed from the east and spoke its name when the day had no name. He marched on with his mother's mother, with his mother's sister, with his father's mother and with his sister-in-law. The uinal was created; the day was created; that was its name. Sky and earth, ebb water, land rocks and trees were created. The things of sea and land were created.

On One Chuen he raised himself up to divinity; he created heaven and earth.

On Two Eb he created the first ebb. It came from the midst of the heavens in the midst of the water. There was neither land, rocks nor trees.

On Three Ben he created all things, as many as there are, the things of the heavens, the things of the sea, the things of the earth.

On Four Ix came the upsetting of heaven and earth.

On Five Men came the making of everything.

On Six Cib it came to pass that he made the first candle. There came light when there was neither sun nor moon.

On Seven Caban the world was first created when we had none.

On Eight Eɔnab his hand and foot were firmly set. Then he picked up the small things on the earth.

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On Nine Cauac hell was first created.

On Ten Ahau wicked men were made to go to hell by Dios supreme that they might not be pointed out.

On Eleven Ymyx he created rocks and trees. This he did within the day.

On Twelve Yk life was created. This the reason he named it life, because there was no death within it.

On Thirteen Akbal he took the water, and raised up the earth, and created two of mankind (or "he raised up the earth, shaped it and became man").

On One Kan he first created anger on account of the evil he had created.

On Two Chicchan came the finding of the evil he saw in the place.

On Three Cimi he ordained death. Our Lord Dios first established death.

(The next day in the series, Four Manik, is missing, but there is a space where it should be.)

Five Lamat was when he established all the seven great waters of the seas.

On Six Muluc came the drowning of all who were submerged, when the world was not yet created. Then came the creation of the word of our Lord Dios when there was no word in heaven, when there were neither rocks nor trees. Then they went to consider and he spoke as follows: "Thirteen quantities (or heaps), seven quantities, one." This he said and he raised up his word when there was no word. Then the beginning was sought by the first day Ahau, but he did not conclude the declaration of his word to them so that they might declare themselves. Then they went into the midst of heaven and joined hands with one another. And then he set them up in the midst of the land and they are the Four Burners.

| | | | |
|---|---|---|---|
| Four Chicchan, | The Burner. | Four Men, | The Burner. |
| Four Oc, | The Burner. | Four Ahau, | The Burner. |

These are the four Ahaus (or kings)

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| | | | |
|---|---|---|---|
| Eight Muluc | Thirteen Yx | Five Cauac | Ten Kan |
| Nine Oc | One Men | Six Ahau | Eleven Chicchan |
| Ten Chuen | Two Cib | Seven Ymix | Twelve Cimi |
| Eleven Eb | Three Caban | Eight Yk | Thirteen Manik |
| Twelve Ben | Four Eɔnab | Nine Akbal | One Lamat |

Thus came the creation of the uinal and of the world. Sky, earth, trees and rocks were set in order. All things were created by our Lord Dios. He was heard when there was neither sky nor earth as he was Dios in the clouds by his own power when he created the whole world. Then there was noise and movement in heaven and before God at the great accomplishment of him who was the ruler.

This is the day count, day by day, in which the count begins in the east.

## NOTES

1. In the Maya text only the capitalization and paragraphing are my own. I have kept the spelling and punctuation of the original, but have inserted a few obvious corrections in parentheses.

2. I am unable to find any definition of the word eb which would explain the expression, "eb water," and "the first eb" which "came from the midst of the heavens in the midst of the water," but I believe it refers to the event pictured on plate 74 of the Dresden Codex where the old goddess with the tiger claws is seen emptying a jar of water upon the earth amid a general deluge of water from the heavens. The day sign Eb is pictured in the stream of water which she pours out of her jar, as has been noted by Seler, Forstemann and others. These writers have explained the picture as depicting the end of the world in the far distant future, but this passage in the Chumayel would indicate that it was one of the events in the course of the creation of the world. The old goddess with the claws may well be the "mistress of the world" mentioned in our text. The Dictionary of Motul defines eb as "escalera." Seler (*Gesammelte Abhandlung*, vol. 1. p. 482), gives eb, ebil, ebal and yebal as meaning a series of indentations, a flight of steps of a stairway.

3. Note the word play on the Maya day names in the following cases:

Ca Eb u mentci yax eb.

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Can Ix uchci u nixpahal caan y luum.
Ho Men uchci u meyah tulacal.
Uac Cib uchci u mentci yax cib.
Uaxac Eɔnab e lahi u kab *y* yoc
Lahcabil Yk uchci u çihçic yk.
Ca Chicchan uchci u chictahal u lobil hibal yilah ychil u uich cahe.
Ox Cimil u tusci cimil
Uac Muluc uchci u mucchahal kopob tulacal.

This use of a homonym or a derivative of the Maya day name in relating the event occurring on that day recalls the many undeciphered glyphs both in the picture manuscripts and on the monuments in which a day sign appears as a component part. It also suggests that this part of the account at least was once recorded in a hieroglyphic text.

4. The expression "Oxlahun tuc, uuc tuc, hun," which I have translated as "thirteen quantities, seven quantities, one," must refer to the thirteen numbers which form the numerical prefixes of the day names. In a list of twenty days, seven of these numbers must be repeated, thus thirteen and seven make one entire series of days. Compare this with the similar expression at the end of the creation story translated by D. Juan Martinez y Hernández. "Oxlahun-pic-ɔac tu uuc u xocan yahal cab," which he translates," "Trece pic-ɔac ú órdenes numerales por siete veces, son el transcurso del principio del mundo." Instead of "thirteen pic-ɔac multiplied by seven," I think this should read "thirteen pic-ɔac added to seven," just as the Maya number ca-tu-kal means twenty-two and not two times twenty. I believe that "tuc,—monton de cosas menudas aiuntadas como sal, tierra, leña ettc" (Dic. de Motul) and pic-ɔac-cab,—numero grande, inumerable (Pio Pérez, p. 277, as quoted in Martínez), are used with approximately the same meaning in the cases above cited.

5. I do not know just what is meant by the four burners but I am convinced that they are closely connected with the time period called the uinal. In the Chilam Balam of Tizimin there is an account of the four burners in which I find the following: "He lai ah tocob lae ti culanob tu chun uinale." "These are the burners which are set at the beginning of the uinal."

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# THE CLASSIFICATION OF AMERICAN LANGUAGES

By FRANZ BOAS

EVER since Major Powell completed his classification of American languages, which was published in the seventh volume of the Annual Reports of the Bureau of (American) Ethnology, and a revised edition of which is contained in the first volume of the Handbook of North American Indians, students of American languages have paid more attention to a better understanding and a more thorough knowledge of the single languages than to classification. Much of the material on which Major Powell's work is based is exceedingly scanty, and it is obvious that more accurate studies will show relationships between linguistic stocks which at the time could not be safely inferred. The classification is largely based on vocabularies. Many of these were contained in old literature and are very inadequate. Others were hastily collected in accordance with the exigencies of the situation, and neither Major Powell nor any of his collaborators, like Albert S. Gatschet and James Owen Dorsey, would have claimed that their classification and the map of distribution of languages could be considered as final.

Of late years, largely through the influence of Dr. Edward Sapir, the attempts have been revived to compare, on the basis of vocabularies, languages which apparently are very distinct, and Drs. Sapir, Kroeber, Dixon, and particularly Radin, have attempted to prove far-reaching relationships.

Since for many years I have taken the position that comparison between American languages should proceed from the study of fairly closely related dialects towards the study of more diverse forms, it seems desirable to state briefly the theoretical points of view upon which my own attitude has been and is still based. As early as 1893 I pointed out that the study of the grammar of American languages has demonstrated the occurrence of a number of



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striking morphological similarities between neighboring stocks which, however, are not accompanied by appreciable similarities in vocabulary. At that time I was inclined to consider these similarities as a proof of relationship of the same order as that of languages belonging, for instance, to the Indo-European family. While further studies, particularly in California, have shown that we may generalize the observations which I made based on the languages of the North Pacific coast, I doubt whether the interpretation given at that time is tenable.

When we consider the history of human languages as it is revealed by their present distribution and by what little we know about their history during the last few thousand years, it appears fairly clearly that the present wide distribution of a few linguistic stocks is a late phenomenon, and that in earlier times the area occupied by each linguistic family was small. It seems reasonable to suppose that the number of languages that have disappeared is very large. Taking our American conditions as an example, we may observe at the present time that many languages are spoken by small communities, and while there is no proof of the recent development of any new very divergent language, there are numerous proofs showing the extinction of some languages and the gradual extension of others. As the area occupied by the Indo-European family has gradually extended and as foreign languages have become extinct owing to its expansion, so we find that Chinese has gradually expanded its area. In Siberia, Turkish and other native languages have superseded the ancient local languages. In Africa the large expansion of Bantu is rather recent. Arabic is superseding the native speech in North Africa. In America the expansion of Algonquin speech has been continuing during the historic period, and several of the isolated languages of the Southeast have been superseded by Creek and related languages. I have discussed this question in another place and have explained my view that probably at a very early time the diversity of languages among people of the same physical type was much greater than it is now. I do not mean to imply by this that all the languages must have developed entirely independently, but rather

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that, if there was an ancient common source of several modern languages, they have become so much differentiated, that without historical knowledge of their growth, the attempts to prove their interrelation cannot succeed.

It should be borne in mind that the problem of the study of languages is not one of classification but that our task is to trace the history of the development of human speech. Therefore, classification is only a means to an end. Our aim is to unravel the history of the growth of human language, and, if possible, to discover its underlying psychological and physiological causes. From this point of view the linguistic phenomena cannot be treated as a unit, but the manifestations of linguistic activity must be studied first each by itself, then in their relations to other linguistic phenomena.

The three fundamental aspects of human speech are phonetics, grammar, and vocabulary. When we turn to their consideration separately, we find, at least in America, a curious condition. The study of phonetics indicates that certain features have a limited and well-defined distribution which, on the whole, is continuous. To give an example: the extraordinary development of the series of *k* sounds and of laterals (*l* sounds) is common to the most diverse languages of the North Pacific coast, while in California and east of the Rocky mountains this characteristic feature disappears. In a similar way nasalization of vowels is absent in the northwest part of America, but it is very strongly developed on the central and eastern plains. The labialization of *k* sounds following an *o* or *u* is widely spread in the extreme Northwest, and infrequent outside of that territory. The study of the phonetics of America is not sufficiently developed to describe in detail areas of distribution of characteristic sounds or sound groups, but it may safely be stated from what we know, that similar phonetic traits often belong to languages which are morphologically entirely distinct; and that on the other hand, very great phonetic differences develop in the same linguistic stock.

The study of the morphology of American languages illustrates also definite areas of characterization. It is, for instance, most

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striking that reduplication as a morphological process occurs extensively on the Great Plains and in the Eastern Woodlands, as well as in that part of the Pacific coast south of the boundary between British Columbia and Alaska. Among the great families of the north it is entirely unknown. Incorporation, which in earlier times was considered as one of the most characteristic traits of American languages, is also confined to certain definite groups. It is characteristically developed in the Shoshoni group, Pawnee, Kutenai, and Iroquois, while north of this region it is either absent in its characteristic form, or only weakly developed. The use of instrumentals, which indicate the manner of action as performed with parts of the body, or by other instruments, shows also on the whole a continuous distribution. It is a fundamental trait of Kutenai, Shoshoni, and Sioux, and in all of them it is expressed in a similar manner. The use of true cases and of locative and similar noun forms occurs among the Shoshoni and some of their neighbors, while in other regions it is rather rare. Of even greater importance is the differentiation between nominal and verbal concepts, and between neutral and active verbs, the distribution of which is somewhat irregular.

Although our knowledge of these phenomena is not by any means adequate, it appears fairly clearly that, when the various features are studied in detail, the areas of their distribution do not coincide.

The study of the vocabulary presents similar conditions. It would seem that the number of loan words in American languages is not as great as in European languages. At least, it is difficult to recognize loan words in large numbers. It is, however, striking that the word categories which appear in neighboring languages are sometimes quite similar. This appears, for instance, in the case of terms of relationship. The remarkable extent to which the use of reciprocal terms of relationship is found on the western plateaus, is a characteristic example. It is intelligible that nomenclature and cultural states are closely related, and, therefore, it seems plausible that similarities in underlying categories of vocabularies will occur where cultural conditions are the same or nearly the same.

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This remark has no direct bearing upon the stems that underlie word formation. To a certain extent they are dependent upon morphological characteristics, at least in so far that non-existent grammatical categories must be supplied in other ways. When, for instance, some languages, like the Eskimo, lack those adverbial elements which correspond to our prepositions (in, out of, up, down, etc.), these must be supplied by special verbs which do not need to exist in languages that abound in locative verbal elements. On the whole, a certain correlation may be observed between the lexicographical and morphological aspects of a language. The more frequently "material" concepts (in Steinthal's sense) are expressed by morphological devices, the more generalized are, on the whole, the word stems, and words are generally formed by limitation of these stems. When we find similar structure, we find, therefore, also a tendency towards the development of similar categories of stems. There are, however, others that are not so determined. It is, for instance, characteristic of many American languages that verbal ideas are expressed by different stems according to the form of the object in regard to which the verb predicates. This feature occurs particularly in verbs of existence and of motion, so that existence or motion of round, long, flat, etc., objects, are differentiated. This feature is prominent, among others, in Athapascan, Tlingit, Kwakiutl, and Sioux.

While I am not inclined to state categorically that the areas of distribution of phonetic phenomena, of morphological characteristics, and of groups based on similarities in vocabularies are absolutely distinct, I believe this question must be answered empirically before we can undertake to solve the general problem of the history of modern American languages. If it should prove true, as I believe it will, that all these different areas do not coincide, then the conclusion seems inevitable that the different languages must have exerted a far-reaching influence upon one another. If this point of view is correct, then we have to ask ourselves in how far the phenomena of acculturation extend also over the domain of languages.

Considering the conditions of life in primitive society, it is

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intelligible how the phonetics of one language may influence those of another one. Many of the American tribes are very small and intertribal marriages are, comparatively speaking, frequent, either owing to peaceful intercourse, or to the abduction and enslavement of women after warlike raids. There must always have been a considerable number of alien women in each tribe who acquired the foreign language late in life and who, therefore, transmitted the foreign pronunciation to their children. It is true that we cannot give definite observations which prove the occurrence of this phenomenon, but it can hardly be doubted that these processes were operative in all those cases where the number of alien women was considerable in proportion to the number of native women. The objective study of languages also shows that phonetic influences do spread from one people to another. The most characteristic example probably is that of the southern Bantu who have adopted the clicks of the Bushmen and Hottentots, notwithstanding the hostility that prevails between these groups.

It is not so easy to understand the development of similar categories of words in neighboring languages. It is undoubtedly true that forms of social and political organization, as well as religious life, have become alike among neighboring tribes owing to a process of acculturation. The similarity in forms of life creates the necessity of developing terms expressing these forms, and will thus bring about indirectly similarity in those ideas that are expressed by words. When we apply this assumption to such concepts as terms of relationship, in which we remain in doubt as to whether the term creates the feeling accompanying the subsummation of an individual under a category, or whether the feeling creates the term, it seems difficult to understand the psychological process that led to the similarity of classification, although the facts of distribution make it perfectly clear that the similarities are due to diffusion. This difficulty is still greater when we deal with the fundamental concepts contained in the ancient stems that underly the modern words. How, for instance, should the habit of mind to classify all motion according to form spread from one language to another?

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Equally difficult to understand is the spread of morphological traits from one language to another. Nevertheless, I am very much inclined to believe that such transfers do occur, and I even consider it possible that they may modify fundamental structural characteristics. An example of this kind is the intrusion of nominal cases into the upper Chinook dialects, presumably due to Sahaptin influence. I believe that the peculiar development of the second third person in Kutenai, which is so characteristic of Algonquin, is also due to a contact phenomenon, because we find hardly anywhere else a similar development of this tendency. Still another case of peculiar parallelism is found among the Eskimo and Chukchi. Notwithstanding the fundamental differences between the two languages, the modern development of the verb with its numerous semi-participial forms, shows a peculiar parallelism. The traits in question are entirely absent in neighboring languages, and for this reason it is difficult to abstain from the conclusion that these similarities must be due to historical reasons.

The distribution of these phenomena the world over is so irregular, that it would be entirely unwarranted to claim, that all similarities of phonetics, classification of concepts, or of morphology, must be due to borrowing. On the contrary, their distribution shows that they must be considered as due to psychological causes such as the unavoidable necessity of classification of experience in speech, which can lead to a limited number of categories only, or the physiological possibilities of articulation, that also limit the range of possible sounds which are sufficiently distinct to the ear for clear understanding.

To give a few examples: it would hardly be possible to claim that the numerous instrumental prefixes of the Haida and those of Shoshoni, Kutenai, and Sioux, are historically related. It is true that Shoshoni, Kutenai, and Sioux form a continuous group to which might be added many of the Californian languages. Considering the continuity of this area and the absence of analogous forms outside, I am strongly inclined to believe that some historical reason must have led to their peculiar development, but it would be difficult to connect historically the Haida with this district.

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In the same way, it would be rash to associate the strong development of glottalized sounds in Chili with the analogous sounds on the Northwest Coast of America; the distinction between neutral and active verbs among the Maya, Sioux, and Tlingit; or the occurrence of three genders in Indo-European and in Chinook.

Our experience in Indo-European and Semitic languages shows clearly that extended borrowing of words may occur and that borrowed words may undergo such changes that their origin can be understood only by historical study. That similar phenomena have occurred in American languages is indicated by the distribution of such words as names of animals and of plants which are in some cases borrowed. Other classes of nominal concepts are not so subject to borrowing on account of the extensive use in many American languages of descriptive terms. Nevertheless, in mixed settlements considerable numbers of borrowed words may be found. An example of this kind is presented by the Comox of Vancouver island who speak a Salish language with a strong admixture of Kwakiutl words, or by the Bellacoola, another Salish people, who have borrowed many Kwakiutl and Athapascan terms. There is no particular difficulty in understanding the process which leads to the borrowing of words. Intertribal contact must act in this respect in a similar way as international contact does in modern times.

If these observations regarding the influence of acculturation upon language should be correct, then the whole history of American languages must not be treated on the assumption that all languages which show similarities must be considered as branches of the same linguistic family. We should rather find a phenomenon which is parallel to the features characteristic of other ethnological phenomena—namely, a development from diverse sources which are gradually worked into a single cultural unit. We should have to reckon with the tendency of languages to absorb so many foreign traits, that we can no longer speak of a single origin, and that it would be arbitrary whether we associate a language with one or the other of the contributing stocks. In other words, the whole theory of an "Ursprache" for every group of modern languages,

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must be held in abeyance until we can prove that these languages go back to a single stock and that they have not originated, to a large extent, by the process of acculturation.

From this point of view I should not be inclined to claim, for instance, that Tlingit and Athapascan are members of the same linguistic family. There is not the slightest doubt that the morphology of the two groups shows the most far-reaching similarities. Since, furthermore, the two languages are contiguous, the inference is inevitable that these similarities must be due to historical causes. It is, however, another question whether we are to infer immediately that these differences are due to the fact that in very early times the two groups had a common "Ursprache." The vocabularies of Tlingit and Athapascan are fundamentally distinct, and it does not seem to me that Dr. Sapir has proved his case of relationship between the two languages by the comparison of a limited number of words that show slight phonetic similarities. The question would remain to be answered, why there should be such fundamental dissimilarities between by far the larger number of words, and the question should still be asked how these dissimilarities are to be explained.

It is true enough that in a comparison of modern Indo-European languages, without any knowledge of their previous history, it might be very difficult to prove relationship—let us say, between Armenian and English—and we might be compelled to adopt a similar conclusion as the one suggested here. Partially this inference would be correct, because our modern Indo-European languages contain much material that is not Indo-European by origin. The fundamental question is whether this material may become so extensive and influence the morphology so deeply that the inclusion of a language in one group or another might become arbitrary. I think it is well worth considering whether the similarities between Finnish and Indo-European, to which Sweet has called attention, may not be due to such a process of acculturation.

To sum up, it seems to my mind that a critical attitude towards our problem makes it necessary to approach our task from three points of view. Firstly, we must study the differentiation of dialects



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like those of the Sioux, Muskoki, Algonquin, Shoshoni, Salish, and Athapascan. Secondly, we must make a detailed study of the distribution of phonetic, grammatical, and lexicographical phenomena, the latter including also particularly the principles on which the grouping of concepts is based. Finally, our study ought to be directed not only to an investigation of the similarities of languages, but equally intensively towards their dissimilarities. Only on this basis can we hope to solve the general historical problem.

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# BOOK REVIEWS

## METHODS AND PRINCIPLES

*Primitive Society.* ROBERT H. LOWIE. Boni and Liveright: New York, 1920. 463 pp.

Anthropology still has the repute of being one of the newer sciences. But when one reflects that between Morgan's *Ancient Society* and Lowie's *Primitive Society* there elapsed nearly half a century without the publication of a single authoritative work in the attempt to pierce the origins of the human social fabric as a whole, one is led to the reflection that anthropology is also a slowly moving science. The occasion seems good therefore to take stock of progress.

It goes without saying that Lowie's book is in every sense modern. It may also be described as thoroughly American and pragmatic. Its method is one to which the ethnologists of France have been somewhat strangely indifferent; those of England with some recent exceptions distinctly averse; those of Germany addicted with fair consistency but often without enthusiasm; whereas only in this country have ethnologists without notable exception subscribed either tacitly or avowedly to its plan. This method is the ethnographic one. That is, it is descriptive instead of primarily interpretative. It is historical in the sense that it insists on first depicting things as they are and then inferring generalizations secondarily if at all, instead of plunging at once into a search for principles. It may not seem historical in the literal conventional sense because the ethnologist's data are not presented to him chronologically. He is therefore compelled to establish his time sequences. This he does by comparisons, especially by taking the fullest possible cognizance of all space factors,—geography, diffusions, distributions. As soon, however, as he has reconstructed his time sequences as well as he may, he follows the methods of the orthodox historian. He describes, giving his product depth through consideration of environmental and especially of psychological factors; but he describes only. It is each unique event that holds his interest, not the common likeness that may seem to run through events but which he finds, as he remains objective, to dilute thinner in proportion as he scrutinizes more accurately, and finally to melt into intangibilities. On the theoretic side this method has been most rigorously and imaginatively worked out in this country by Boas,



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who has demonstrated its inevitable applicability in feature after feature of human civilization. On the constructive side Wissler's *American Indian* marks what is easily the most comprehensive and soundest historic synthesis yet attempted along these lines. Lowie's book takes in a sense an even wider range, but applies the method to the social institutions of all the peoples of the world whom we are wont to call uncivilized.

The several chapters, besides the introductory and concluding ones, are on Marriage, Polygamy, the Family, Kinship Usages, the Sib, the History of the Sib, the Position of Women, Property, Associations (societies), the Theory of Associations, Rank, Government, and Justice. Being attached to no preformed opinion and noted as a student particularly free from bias, Lowie never goes farther than his facts warrant. The result is a double impression: on the one hand of the endless diversity of institutions; on the other, of the uniformity of human motives and social conduct among civilized and uncivilized peoples.

On the reader who makes his acquaintance with ethnology through this book, the effect must be overwhelming. He will realize the immense multiplicity of cultural phenomena. He will feel that these are as manifold and irregular in primitive as in civilized society and *vice versa;* and that subsumable generalizations fade more and more into the background as the mass of facts is courageously faced and honestly dissected. About all that remains of generic principles, when Lowie has completed his analysis, is the demonstration of certain tendencies that run unexceptionally through civilization but are at the same time so modified in actuality that there is nothing interpretative to be done with them. These tendencies stand revealed as mere boundaries within which the play of cultural forces is confined.

Thus, Lowie sums up his discussion of the family: "In short, the bilateral family is none the less an absolutely universal unit of human society." Inasmuch as there has been a prevalent inclination (due probably in large measure to the fact that the family is bilateral in our own civilization) to seek and find an essentially unilateral family in primitive society, this conclusion is of definite corrective value. It needs no argument, however, to prove that a principle of this nature cannot be used as a tool with which to shape other generalizations. It is much as if we confirmed the validity of the principle that all houses are built on foundations. This might be an extremely important affirmation if a theory held sway that houses could be built without foundations. But no theory of the necessity of foundations will give a working plan

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for the building of houses nor explain why, although all houses possess foundations, they possess such varied form, structure, and size.

Similarly as regards the clan:

There is no fixed succession of maternal and paternal descent; sibless tribes may pass directly into the matrilineal or the patrilineal condition; if the highest civilizations emphasize the paternal side of the family, so do many of the lowest; and the social history of a particular people cannot be reconstructed from any generally valid scheme of evolution but only in the light of its known and probable cultural relations with neighboring peoples.

Or again as regards the status of women:

That neither this superstitious sentiment nor man's physical superiority has produced a far greater debarment of primitive woman, that she is generally well treated and able to influence masculine decision regardless of all theory as to her inferiority or impurity, that it is precisely among some of the rudest peoples that she enjoys practical equality with her mate,—these are the general conclusions which an unbiased survey of the data seems to establish.

On societies:

It follows that the search for all-embracing laws of evolution on the model of Morgan's or Schurtz's schemes is a wild-goose chase and that only an intensive ethnographic study in each cultural province can establish the actual sequence of stages.

As regards law and government:

The majority of primitive communities recognize not merely wrongs inflicted by individuals upon individuals and precipitating a dispute between their respective kins, but over and above the law of torts there is generally a law of crimes, of outrages resented not by a restricted group of relatives but by the entire community or its directors. The conclusion . . . shows the reality of the territorial unit for certain specific social aims. . . . The territorial bond must then be considered as one of the social ties occurring concomitantly with others in the simpler stages of civilization.

These, accordingly, are some of the formulations of sane ethnological research, of the historical method applied to undated data; formulations agreeing with those which other workers have reached in special studies, and which every modern ethnologist of the historical school will readily subscribe to.

It goes almost without saying, then, that Lowie's book is indispensable to whose who desire a fair summary of the facts of primitive society, presented in digest and proportion, with convincing evidence and admirable lucidity. This is a book which those who entertain schemes of social progress will come to hungrily if they recognize the value of possessing a knowledge of the past for construction of the future. It is

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also a book which every sociologist, if his sociology is to be more than a series of spun-out hypotheses, will have to take cognizance of. And finally it is a work whose point of view and conclusion cannot be disregarded in the teaching of the social sciences. In short, *Primitive Society* is a clear and fair representative of what modern ethnology has to offer.

If now we revert attention from the success of the book as the logical exemplification of a method to that method itself, what can be said of the value of this method? This admission seems inevitable: that though the method is sound, and the only one that the ethnologist has found justifiable, yet to the worker in remote fields of science, and to the man of general intellectual interests, its products must appear rather sterile. There is little output that can be applied in other sciences. There is scarcely even anything that psychology, which underlies anthropology, can take hold of and utilize. There are in short no causal explanations. The method leads us to the realization that such and such has happened on such and such an occasion. Human nature indeed remains the same with its conservatism, inertia, and imitativeness (p. 436). But the particular forms which institutions assume evidently depend on a multiplicity of variable immediate factors, and if there are common and permanent factors they either cannot be isolated or remain as vague as the three trends mentioned. In essence, then, modern ethnology says that so and so happens, and may tell why it happened thus in that particular case. It does not tell, and does not try to tell, why things happen in society as such.

This default may be inevitable. It may be nothing but the result of a sane scientific method in a historical field. But it seems important that ethnologists should recognize the situation. As long as we continue offering the world only reconstructions of specific detail, and consistently show a negativistic attitude toward broader conclusions, the world will find very little of profit in ethnology. People do want to know why. After the absorption of the first shock of interest in the fact that the Iroquois have matrilineal clans and that the Arunta have totems, they want to know why they have them when we have not. The answer of ethnology as typified by Lowie is in substance that there are tribes fully as primitive as the Iroquois and Arunta who are like ourselves in that they possess neither clans nor totems. But again the justifiable question obtrudes: Why do some primitive cultures develop clans and totems while others fail to? And we say that we do not know or that diffusion of an idea did or did not reach a certain area. Now it may be

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contended that such questionings are naive. Yet they occur and will recur. And it would seem accordingly that ethnologists owe it to their consciences to realize clearly how limited the scope of their results is, how little they satisfy the demand—be it justified or simple—for broad results, or offer formulations that will prevent the average inquirer's relapse into the comforting embrace of easy and unsound theories. Such a realization is not marked in Lowie's volume.

And finally, however firmly scientific ideals may hold us to the tools which we use, we must also recognize that the desire for the applicability of knowledge to human conduct is an inescapable one. That branch of science which renounces the hope of contributing at least something to the shaping of life is headed into a blind alley. Therefore, if we cannot present anything that the world can use, it is at least incumbent on us to let this failure burn into our consciousness.

Serious as this comparative sterility is, it is yet preferable to the point of view which recognizes the demand but attempts to satisfy it with conclusions derived from shallow thinking under the influence of personal predilection. After all honesty is the primary virtue, and Lowie's soberness is a long advance on Morgan's brilliant illusions. But one sometimes sighs regretfully that the honesty of the method which is so successfully exemplified here is not stirred into quicker pulse by visions of more ultimate enterprise.

A. L. KROEBER

*Pagan and Christian Creeds; Their Origin and Meaning.* EDWARD CARPENTER. New York: Harcourt, Brace and Howe, 1920. Pp. 319.

Mr. Carpenter is best known for his book on *Civilization; Its Cause and Its Cure*. In that book he has a chapter entitled "Exfoliation," which in all good conscience may be taken as the forerunner of Bergson's *Creative Evolution*. The thesis of that chapter is that evolution is as much the unfolding of potentialities within the individual as it is the result of the impact upon him of external forces—that he evolves himself. In *The Art of Creation* Carpenter has carried out this idea with emphasis upon varied phases of the self which he believed exemplified it. Now, in *Pagan and Christian Creeds*, he gives this idea application in the realm of religious ideas. His thesis is about as follows:

There is a remarkable similarity between the religious beliefs of peoples, and this similarity is none the less evident if we take for comparison those creeds usually supposed to be at the opposite poles, namely,

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Christianity and primitive religion. There is the importance of ritual; of initiations or conversions, designed to effect much the same thing; of sacrificial offerings; of borrowings from related cultures.

What is the explanation of this uniformity? It lies in the essential unity of man, whatever his social environment may be. Its expression is determined by the environment and by the times in which he lives, but the will to struggle with nature and to comprehend the mysteries of life is inherent in man and must find its outlet. As his intellectual development proceeds he assumes new attitudes in his endeavor to interpret and to utilize nature and her message. It is, Mr. Carpenter thinks, time that Christianity shed some of the cruder interpretations and adopted points of view that harmonize better with the renaissance of learning.

In the evolution of the religious consciousness we can recognize three stages: The first is that in which man does not perceive himself as a creature essentially separate from the rest of nature; the second is the stage of clearly defined self-consciousness and exaltation of the self; the third is, somewhat after the Hegelian principle, a reconciliation of the inconsistencies existing between the first and the second, a stage in which man has self-consciousness, a clear perception of the external world, and has effected a unity between the two.

> To some such unity-consciousness we have to return; but clearly it will be—it is not—of the simple inchoate character of the First Stage, for it has been enriched, deepened, and greatly extended by the experience of the Second Stage.

The moral of the book—for I think it may be said to have a moral—is that man will attain freedom by recognition of the chains that have bound him. For then he can break them and achieve that freedom which is the goal of individual life.

> He will realize the inner meaning of the creeds and rituals of the ancient religions, and will hail with joy the fulfilment of their far prophecy down the ages—finding after all the long-expected Saviour of the world within his own breast, and Paradise in the disclosure there of the everlasting peace of the soul.

As always, Mr. Carpenter is forceful and stimulating. At the age of seventy-five he shows all the mental vigor of middle life, and the liberality of his thought has not abated one jot. No one will read him without feeling repaid. There are many new angles on the interpretation of religious rites or beliefs, and the author has injected new life into many of the old problems. If we disagree with him on many issues he will at least force us to find new arguments for our old conclusions.

W. D. WALLIS

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*Messiahs; Christian and Pagan.* WILSON D. WALLIS. Boston: Richard G. Badger, 1918. 276 pp.

In this volume Dr. Wallis has assembled a vast mass of interesting and curious information bearing on a problem of perennial attractiveness in the study of religion. Both the extent and the diversity of his reading are astonishing. It is regrettable, however, that the innumerable references are nowhere brought together in an alphabetic list, where it would have been best perhaps to divide them into several groups. Indeed, the publisher's bibliographical technique is objectionable. Most of the references are inserted square-bracketed into the body of the text; in one chapter, for no ostensible reason, footnotes in conjunction with the other scheme; and thereafter they persist less prominently until the end.

The strength of Dr. Wallis's book lies in two directions. As already indicated, he describes a very extensive group of data not less interesting to the student of culture because most of them are culled from the annals of the more complicated societies. Secondly, he attacks the old problem of the relation between the individual and society. Not that he solves the enigma. But his insistent formulation of it merits appreciation, and some of his general reflections on the subject are full of common sense.

On the other hand, there is a disproportion between the descriptive and interpretative parts. It seems to me, at all events, that as a rule Dr. Wallis does not come to grips sufficiently with his information but is inclined to discuss it only from the most abstract points of view. This gives a certain pallor to his generalizations even when they are not challengeable.

But whether this criticism be justified or not, the concluding chapter certainly strikes one as disjointed, and its closing section on "The Mission of Jesus" seems out of place in what purports to be a summary of the generic traits of the Messianic idea.

ROBERT H. LOWIE

## NORTH AMERICA

*Preliminary Account of the Antiquities of the Region between the Mancos and La Plata Rivers in Southwestern Colorado.* EARL H. MORRIS. (Thirty-third Annual Report of the Bureau of American Ethnology, pp. 155–206.) Washington, 1919.

This is another of the compact, lucid, and useful reports which we have come to expect from Mr. Morris. The work was done in 1913 and 1914, with less intensive exploration than the author would have liked to conduct. Two types of prehistoric culture were encountered. The

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later is represented in Johnson Canyon, a tributary of the Mancos on the opposite side from the flows that drain the Mesa Verde. This is but an extension of the Mesa Verde cliff-dweller culture and at least approximately contemporaneous. On the mesas at the head of Johnson and parallel southern canyons were discovered the remains of an earlier culture characterized by the absence of terrace architecture and corrugated pottery. Rooms were usually sunk into the ground, lined with upright slabs, and the walls carried up by means of mud-coated poles. They were grouped into houses of as many as a hundred contiguous rooms. Corn was grown. The pottery is remarkably variable in form, crude technically, and more often than not unpainted. The designs appeal to the reviewer as being more similar to those of modern Yuman pottery than any others in the Southwest. Obviously this is a pre-pueblo culture more or less allied to that sometimes called "slab-house."

It is out of investigations like this that the history of the region is being pieced into an ever more coherent and well-founded whole. The author cannot be commended too highly for the terse excellence of his descriptions; and his illustrations illustrate.

A. L. KROEBER

*Uses of Plants by the Indians of the Missouri River Region.* MELVIN RANDOLPH GILMORE. (Thirty-third Annual Report of the Bureau of American Ethnology, pp. 43–154.) Washington, 1919.

This is the third of a series of ethnobotanical monographs brought out by the Bureau of Ethnology, and covers the subject for the Plains as the Zuñi and Tewa studies have dealt with the Southwest. The region specially represented is Nebraska, the tribes from which data were obtained by the author being the Teton Dakota, Omaha, Ponka, and Pawnee. Eastern tribes as far as the Atlantic coast are adduced in comparisons, but there is almost no reference to the ethnobotany of the Southwest, intermountain region, or Pacific coast. The first-hand information from the four Nebraska tribes seems to have been gathered reliably and accurately. There are some interesting notes on cultivation of plants that it has recently become difficult for the Indians to secure wild; and an argument that the watermelon is part of indigenous American agriculture. Some weight must be given the evidence presented on this point; but there can be no final verdict until an unprejudiced monographic botanical judgment is rendered.

Like the numerous previous ethnobotanical studies of the compilatory kind, this one leaves the impression of having been made in

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satisfaction of some collecting impulse rather than as an attempt to solve a problem. Such works have their value; but it is well to recognize that it is only as providers of the raw materials of science. If the author's interest had been really ethnological or historical, he would scarcely have failed to go into the details of native maize culture, to ascertain its ramifications and adaptations, to learn, for instance, precisely how Indian agriculture has developed or utilized the varieties of the plant. A comparison with the Zuñi and Tewa studies, or the Californian ones of Barrows and Chestnut, might have led to findings of wider import. It could at any rate have been attempted: even a negative result would be of value. Then there is the question, which is almost always avoided in work of this character, of what plants the natives might have used but did not. That they were ingenious in finding things to eat and drug themselves with is a fact that may still astound the unlearned, but will not cause the least ripple among scientists. We want to know the limits of their ingenuity, and the causes of the limitation; which means a collation and integration of the plant lore and plant employment of a tribe with the whole of its culture. That four tribes used 170 different plants will have quite different significance according as the flora of their region numbers 200 or 2000 species. Ethnobotanical studies of the sort to which this undertaking and most of its predecessors belong have got into a way of being passed over by ethnologists with polite commendation and the thought that botanists may be interested in that kind of thing, while botanists seem to dismiss them as probably useful to ethnologists.

A. L. KROEBER

## SOME NEW PUBLICATIONS

Goddard, Pliny Earle. Notes on the Sun Dance of the Sarsi. Notes on the Sun Dance of the Cree in Alberta. (Anthropological Papers of the American Museum of Natural History, vol. XVI, pt. 4, pp. 271–282, 295–310, 3 figs.) New York, 1919.

Hrdlička, Aleš. Physical Anthropology. The Wistar Institute: Philadelphia, 1919. 8vo. 164 pp., 4 pls.

Lowie, Robert H. Sun Dance of the Shoshoni, Ute, and Hidatsa. (Anthropological Papers of the American Museum of Natural History, vol. XVI, pt. 5, pp. 387–410, 411–431, 4 figs.) New York, 1919.

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Nordenskiöld, Erland. The Changes in the Material Culture of Two Indian Tribes under the Influence of New Surroundings. Göteborg: Elander, 1920. Pp. VXI, 245.

Parsons, F. G. Anthropological Observations on German Prisoners of War. (Journal of the Royal Anthropological Institute, vol. XLIX, 1919; pp. 20–35.)

Rivet, P. Bibliographie Américaniste 1914–1919. (Journal de la Société des Américanistes de Paris, N.S., vol. XI, 1919; pp. 667–738.)

Skinner, Alanson. The Sun Dance of the Plains Cree. Notes on the Sun Dance of the Sisseton Dakota. (Anthropological Papers of the American Museum of Natural History, vol. XVI, pt. 4, pp. 283–293, 381–385.) New York, 1919.

Sullivan, Louis R. Anthropometry of the Siouan Tribes. (Anthropological Papers of the American Museum of Natural History, vol. XXIII, pt. 3, pp. 81–174, 7 figs., 74 tables.) New York, 1920.

Thurnwald, Richard. Die Psychologie des Totemismus. (Anthropos, vols. XII–XIII, 1917–1918, pp. 1094–1113.)

Wallis, W. D. The Sun Dance of the Canadian Dakotas. (Anthropological Papers of the American Museum of Natural History, vol. XVI, pt. 4, pp. 317–380.) New York, 1919.

Waterman, T. T. Yurok Geography. (University of California Publications in American Archaeology and Ethnology, vol. XVI, no. 5, pp. 177–314, pls. 1–16, 1 fig., 34 maps.) Berkeley, 1920.

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# DISCUSSION AND CORRESPONDENCE

## Who Made the Kayenta-National Monument Ruins

The Kayenta region, Arizona, and the Navajo National Monument to the west of it are dotted with extensive ruins, among which are the cliff ruins of Keetseel and Betatakin. These regions are as yet scarcely touched. The only published works extant are Dr. J. Walter Fewkes' "Preliminary Report on a Visit to the Navajo National Monument, Arizona," Bull. 50 of the Bureau of American Ethnology, and Kidder and Guernsey's "Archeological Explorations in Northeastern Arizona," Bull. 65 of the same bureau. What peoples made the ruins is a matter of considerable speculation.

Pictographs on the canyon walls undoubtedly often show Hopi maidens with their whorled hair representing the pumpkin blossom of fertility. My Hopi helper, Clarence Taptuka, and his wife's relatives also readily identify the glyphs on the rock walls near the ruin on Man's Head point northwest of the Marsh Pass Indian school to be the signs of the Snake, Spider, and Rabbit clans of their people. Ceremonial object No. 65 of Kidder and Guernsey (*op. cit.*, p. 145) they identify as an ear pendant of their people, representing the spreading pumpkin blossom, used in the Kachina dances and in the Butterfly ceremonies. Also they readily identify the sunflowers and cones, found by Kidder and Guernsey and the bird figured by them (above, pp. 143–147), as paraphernalia used by their people.

Mr. Taptuka states:

> The bird is used in the Kachina dances. It is usually placed on some conspicuous place on the dancing mask. The cones are used to represent ears on the mask, also worn in certain Kachina ceremonies (as Tacab (Tenebiji) is dressed on plate XXVI of the Twenty-first Annual Report, Bureau of American Ethnology). Usually only one is worn on the mask, on the right side. False hair is then wrapped around this pretended ear and let fall over it in front so as to obscure it and the string attachments that hold it to the mask. The wearing of one ear on a mask is to illustrate a myth of the long ago which states that a certain maiden, who was making her toilet, had one whorl of her hair done up to represent the pumpkin blossom when she was attacked by an enemy from whom she escaped with her hair only half arranged.
>
> The sunflowers are used only in the Bean ceremonies. They are used somewhat like a forehead star is sometimes used by white people. They are used in



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the initiation ceremonies into the order. The wearer is a man impersonating a woman and is so dressed. In the dance the women and men line up in the kiva facing each other like white people do in the 'Virginia Reel,' the men occupying the right side of the room as one faces the banquette (visitors' place in the hall[1]), the women the left. The men impersonating women line up with the women. In the dance the two at the foot of the rows, the end farthest from the banquette, step to the center of the room and clasp hands (like Powamu and Sowugti are clasping hands, as shown on plate XIV of the above ethnological report). They then dance forward to the front of the room to the sipapu hole in the floor in front of the fire-pit. They then retrograde in a backward dancing movement to the starting point. Then again they dance forward to the sipapu hole in the kiva, after which they separate, each going to his or her respective side. While this couple are thus dancing, the columns are dancing in a slight shuffle, side movement to the rear. As they dance, the men wave rattles in their right hands and bunches of cedar twigs in their left. While the women wave longer cedar twigs in their left hands only. Also as the respective partners come together for the central dance, the man gives his partner a piece of corn bread baked in corn-husks, so tied with yucca as to much resemble a white man's fancy necktie. This the lady accepts and thrusts into the bosom of her dress, or within a fold of her blanket, provided she wears one. A 'set' lasts through the singing of a chanted song. Several sets are thus danced. Then the participants repair to a neighboring kiva and enact the same ceremony again. Thus they go from kiva to kiva and perform until the close of the night. The corn bread, which has been collected now and then, is then eaten without the kiva of each respective clan.

Hopi myths and traditions also indicate that the Horn, Flute, and other clans of their people once lived in the Kayenta-National Monument country and the region westward from there to the Navajo Mountains and the Grand canyon. Again, some of the ruins look like Jemez structures, and, as it is a matter of historic record that the Jemez fled to the Navajo country when defeated by the Spaniards in 1696, it is quite possible that some of the villages now in ruins were made by them at this time.

It would seem that at least a part of the more ancient ruins were made by Hopi clans and some of the recent ones by the Jemez and probably other Rio Grande village peoples who fled to the Navajo country during the troubled times between the Pueblos and the Spaniards.

ALBERT B. REAGAN

## STONE INSCRIPTIONS AND ESCUTCHEONS

BERNARDO DE AZEVEDO DE SILVA RAMOS announces in Manaos, in a letter to the historian Rocha Pombo in Rio de Janeiro, that he expects

[1] The parentheses are the writer's.

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soon to publish the results of his investigations in the field of Brazilian stone inscriptions. In said letter, which was published in the daily press, Ramos calls attention to the fact that he has succeeded in deciphering the inscriptions which appear in vols. I and L of the "Revista do Instituto Historico do Brasil;" *i.e.*, those of the Gavea mountain and of the deserted city in the *Hinterland* of Bahia; further all the inscriptions which were copied by P. Francisco de Menezes as well as those of the "Pedra Lavrada" (carved stone) of Parahyba, etc. He has also deciphered many symbols and inscriptions on ceramic wares as well as an inscription on the rocks which he recently discovered on the banks of the Rio Urubú. Ramos regards all these inscriptions as prehistoric. In the work which he is about to publish he will add the picture inscriptions of Rocky Dell creek (U. S. A.) and that of the island of Lagosta in Dalmatia which were published in "Le Tour du Monde," I. Sem. 1860, and which he likewise claims to have succeeded in deciphering. He recognizes a great similarity between these inscriptions and those of Brazil. Also the inscriptions of the Rio Chalinga (Chile) which were discussed in Vol. 28, Ser. II of the publications of the Scientific Society of Chile he has deciphered paleographically. Ramos' work will comprise two volumes of 520 pages each and will contain 875 figures. This work, in which the author discusses with great clearness the definite significance of hundreds of Brazilian stone inscriptions will naturally be awaited with much interest by the scientific world.

In a paper presented in 1916 before the 5th Brazilian Geographical Congress of Bahia by the engineer Dr. Theodore Sampaio he discusses the stone inscriptions in the valley of the Paraguassú. In his introduction he calls attention to the two opposite viewpoints obtaining in the interpretations of stone inscriptions of South America on the part of anthropologists and Americanists. Certain ones, among whom we might mention Richard Andrée, Garrick Malley, Theodor Koch-Grünberg and the native investigator Dr. Alfredo de Carvalho, are of the opinion that these inscriptions possess no symbolic value, but are simply scribblings made on the rocks as a matter of diversion by the natives in order to beguile their many hours of leisure. Other investigators regard these rock inscriptions as symbolic representations dealing with the history of the American peoples; said representations still being meaningless to us due to the fact that we lack as yet the key to their solution. In part, Dr. Sempaio discusses colored representations of the most varied types. Of these he has presented many reproductions. His conclusions, which, to be sure, he declares to be by no means final, are the following:

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1. The paintings on stone in the caves of the Serroto do Pintor and the Casa de Pedra (which were used by the Maracás Indians as burial places) evidently served the purpose of indicating the genealogy or posterity of the dead.

2. These varicolored paintings, which frequently are located at a considerable height, so much so, in fact, that special arrangements must have been made use of in order thus to locate them, are not the result of a mere idling away of the time but are due to preconsidered work and carried out with a definite purpose in view.

3. The inscriptions which have been studied, at least those of the Serroto do Pintor, are to be dated at about the time of the conquests by the Europeans or even later.

On examining Sempaio's pictures of drawings by Brazilian Indians we are stuck by certain forms which occur repeatedly and which remind us of the prehistoric ones of Altamira and those of the French caves. This applies especially to the digitated or pectiniform figures, which show here 3, 4, 5, 6, or even more perpendicular lines and are provided with a border, so that the entire figure reminds one of a shield. According to Dr. Sampaio these drawings, which he likewise designates as *escudetes* (shields) were intended to indicate the number of the offspring of those whose remains were buried there.

While we have to admit the close resemblance of the figures found in the cave of Altamira with those of Brazil, we nevertheless cannot agree so readily with Dr. Sampaio's explanation. In no way do the drawings of Altamira, which are primarily representations of animals, indicate the presence of a burial place. The so-called digitated figure of Altamira, which is located above the representation of an animal, reminds one of a craftman's "trademark." Be it remembered that an individual, by simple strokes or indentations on a weapon or utensil, could thereby indicate his ownership. It is not at all impossible that this type of figures, to which different ones were added later, could indicate the mark of ownership, not only of individuals, but also of entire clans. In the case of the pectiniform drawing of Altamira and that of the Indian drawings in the valley of the Paraguassú, we are probably dealing with such tribal marks. The same mark is found among those of the manufacturers of steel wares in Solingen. Let us also note that even today many families have on their escutcheons lines, (*i.e.*, pales) similar to those found in the drawings here in question. In the Spanish cave as wel las among the Brazilian inscriptions we find also certain scalariform drawings. Even today the cattle raisers of Ceará frequently use marks which they call *escadas* (ladders) in branding their herds. These marks consist of two horizontal lines crossed by two perpendiculars. The

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Chinese character for "eye" (pronounced *mo*) has likewise the form of a small, upright ladder. Scalariform marks frequently occur on the escutcheons of western Europeans, just as, likewise, the human eye is at times represented. These signs evidently signify man, the human being. The same may be said of various forms of crosses and "T"-shaped signs. They represent man, just as today the illiterates represent their person on a document by the making of a cross. A great number of present day family names contain the designation "man" in a great variety of forms. Probably, therefore, the old inscriptions or escutcheons, which were originally indications of names, represent this designation in manifold forms. The collection of Brazilian figures, which was greatly increased by the ceramic examples of Marajó (cf., Archivo do Museo Nacional, Rio de Janeiro, Vol. II, 1885) contain to a surprising extent the following forms, which are commonly found also on European escutcheons: *viz.*, angles, crenellated and horseshoe or antler-like forms, three or four-leaved drawings, etc. Of importance also in the case of the Brazilian figures is the phallus, which has its counterpart on various western European coats-of-arms and is, in the latter case, at times popularly supposed to represent candles, arrow-points, etc. It seems that also in western Europe there remained evidences of the veneration of the generative forces as was commonly the case among ancient peoples. In Altamira there are further many hand impressions, many of them in a mutilated condition. They were perhaps originally intended as marks of recognition and served the same general purpose as fingerprints do today. Whether the arms, hands, claws, etc., appearing on escutcheons were at first intended to serve a similar purpose is not so readily to be accepted as a fact, and yet it is quite possible that this was the case. It is certain, at least, that many of the forms appearing on certain coats-of-arms are exceedingly ancient. If we should succeed in proving an actual connection between the prehistoric drawings such as those under discussion and the oldest forms appearing on our escutcheons, a matter which must be left to further investigations, then a new field would be opened to students of prehistoric inscriptions and heraldry. The many so-called secondary signs in the forms of suns, hearts, rings, arrows, birds, dragons, etc., which appear on coins and seals seem to point to such a connection.

FREDERICO SOMMER

SÃO PAULO, BRAZIL,
September, 1920.

(Translated and communicated by B. F. Schappelle, Univ. of Pa.)



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# ANTHROPOLOGICAL NOTES

## Anthropology in the Pan-Pacific Scientific Congress, Honolulu, August 2 to 20, 1920

The first Pan-Pacific Scientific Congress for the consideration of research in the Pacific met in Honolulu last August at the invitation of the Pan-Pacific Union. The program and preliminary organization were placed in the hands of the National Research Council of the United States and were referred to the Committee on Pacific Exploration, of which Dr. J. C. Merriam is chairman. The members of this committee, not being able to attend the Congress, delegated their responsibilities to Dr. H. E. Gregory and Dr. Clark Wissler, to act as a sub-committee for the arrangement of the preliminary organization and the program. The plan submitted by this sub-committee was adopted by the Congress as its scheme of organization, declaring itself to be international in scope and representing the scientific men of all the nations in and around the Pacific.

Sixty delegates were in attendance, representing the United States, Territory of Hawaii, the Philippine Islands, Canada, Japan, England, Australia, and New Zealand. It was proposed that the members of this Congress should constitute a general committee for the formulation of a research program for the Pacific with a view to coördinating the scientific activities of all the nations concerned. To facilitate this program a number of sectional committees were formed, one of which was for anthropology. This section, in conformity to the policy of the Congress, undertook the formulation of a plan for the development and coördination of anthropological research in the islands of the Pacific, particularly in Polynesia. Polynesia was emphasized because the section received a formal request from the Trustees of the Bishop Museum in Honolulu for detailed recommendations for the organization of their own investigations in Polynesia for which funds have recently been provided. It proved impossible to complete the work of the section during the three weeks allotted, but provision was made for the final formulation of its recommendations under the direction of the section officers.

The Congress held daily sessions, giving the entire morning of each



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day to the principal sciences concerned in Pacific research. These sessions were attended by the whole Congress and the discussions freely participated in. One entire morning was given to anthropology, the presiding officer being Dr. Frederick Wood-Jones, University of Adelaide. The formal presentations were as follows:

Clark Wissler: The Chronological Problem in the Pacific.

A. L. Kroeber: Peoples of the Philippines.

L. R. Sullivan: The Racial Problem in Polynesia.

A. M. Tozzer: Race Mixture in the Pacific.

J. F. G. Stokes: Distribution of Culture Traits in the Pacific as Illustrated in Feather-Work.

T. G. Thrum: Polynesian Archaeology.

The anthropological representation in the Congress was as follows:

United States: Clark Wissler, A. L. Kroeber, A. M. Tozzer, Gerard Fowke, L. R. Sullivan, R. T. Aitken.

Territory of Hawaii: W. T. Brigham, J. F. G. Stokes, T. G. Thrum.

Australia: Frederick Wood-Jones.

New Zealand: J. Allan Thompson.

Philippine Islands: No anthropologist accompanied the Philippine delegates, but the subject was represented in the section by E. D. Merrill, Director of the Philippine Bureau of Science.

Japan also sent no anthropologist, but the work of Japanese anthropologists was presented by Dr. N. Yamasaki, Professor of Geography, Tokyo Imperial University. Dr. K. Kishinouye, the celebrated Japanese zoölogist, who has made a special study of Japanese shell-heaps, also took a prominent part in the meetings of the section.

Mr. Diamond Jenness was made Associate Ethnologist in the Division of Anthropology of the Geological Survey of Canada on October 1, 1920. Mr. Jenness received his anthropological training at Oxford University and shortly after carried on researches among the Melanesian-speaking peoples of British New Guinea in 1911-1912. The anthropological results of this trip are now being published by the Oxford University Press under the title of "The Northern D'Entrecasteaux." In 1913–1916 Mr. Jenness served on the Canadian Arctic Expedition as one of the two anthropologists engaged by the Dominion Government to carry on ethnological, archaeological, and linguistic researches among the Eskimo of the various regions visited. Owing to M. Beuchat's lamented death in the Arctic the anthropological duties of the Expedition devolved almost entirely on Mr. Jenness. Since his return from the field he has

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been preparing various monographs on the results of his expedition. The introductory monograph, "The Life of the Copper Eskimos," is ready for the press. Other papers, particularly those dealing with folklore, physical anthropology, and the comparative study of Eskimo cat's cradles, are in an advanced state of preparation. Special papers on linguistics, archaeology, and material culture are to follow. The bulk of Mr. Jenness' material was obtained from the Copper Eskimo of Coronation gulf and Victoria island, but a great deal of it, particularly the archaeology and linguistics, belongs also to northern Alaska.

MRS. AGNES DONOHUGH is giving courses in ethnology in the Kennedy School of Missions, Hartford, Conn. She is also giving a course on African native life in the Hartford Theological Seminary. The courses in this institution are intended for better fitting missionaries to undertake work with the natives. Mrs. Donohugh has recently been elected a Fellow of the Royal Anthropological Institute of Great Britain and Ireland.

AT the Commencement of the University of Pennsylvania, June 16, Alfred I. Hallowell received the degree of M.S., and W. Leon Godshall that of M.A.

DR. J. W. FEWKES, Chief of the Bureau of American Ethnology, has continued his archaeological investigations on the Mesa Verde National Park, Colorado, during the past summer, the Bureau of American Ethnology and the National Park Service acting in collaboration.

DURING the month of June, Mr. J. N. B. Hewitt of the Bureau of American Ethnology conducted investigations among the Oneida Indians of Wisconsin, the Seneca of New York, and the Mohawk and Onondaga of Grand River, Canada.

*Man* (September 1920, p. 139) reports the death of Rodolfo Livi, famous among physical anthropologists for his monumental *Anthropometria militare.*

DON SAMUEL A. LAFONE QUEVEDO died on June 18, 1920 at La Plata at the age of 86. As an archaeologist and a student of linguistics he has played a large part in the scientific life of the Argentine. His investigations embraced the whole northern part of his country as well as parts of Bolivia. To him we owe the first ethnological investigations of the Calchaqui and Tucumán valleys. He opened the way for the investi-

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gations of Ambrosetti, Debenedetti, and others. He was also an educator and played a great part in the Department of Public Instruction in Catamarca. He was connected with the Faculty of Philosophy and Letters in the same city. In 1906 he was made Director of the Museum of La Plata and later became a member of the Faculty of Science in the University of La Plata. In his death the country has lost one of its most ardent scholars.

THE following corrections to Prof. Boas' article entitled "The Social Organization of the Kwakiutl" should be noted:

On p. 113, line 6, omit "whom we may call IIC*d*10."

Line 10, read IIIC*d*'12 instead of IIIC*d*'15.—In the explanation of Fig. 3 under No. 7, read DzEWx·q!—, for EzEnx·q!—; and No. 13 ALĒ—for Alē.

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